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## THE NECESSITY FOR A NEW STANDPOINT IN SLEEP THEORIES.

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Sleep theories have suffered from two great lacks, lack of community of work and lack of systematization. Although many valuable researches have been made upon the physiological conditions of sleep, the work so far has been scattered and carried on, for the most part, by specialists in their own departments, without a proper realization of the breadth of the problem or the interrelation of its various phases. Recent theories have shown a tendency to combine elements from the three main lines of investigation followed, but such systematization has not been carried far. It is the object of the present article to subject the factual basis of the vasomotor, the chemical, and the histological theories of sleep to as thoroughgoing and impartial criticism as possible, and to show the exact relation of each to the problem in hand. In attempting this it has been necessary to treat the subject from the evolutionary standpoint. The theory presented in the concluding chapter claims nothing of finality. It is not our purpose to add another to the already too numerous theories of sleep, but rather to gain a new point of attack,—which we believe to be the only point of attack that offers promise of a solution of the problem.

### I. THE CIRCULATION THEORIES.

The phenomenon of sleep has never received the attention that, from its importance to life, it deserves. As with all scientific problems which have been of general interest to mankind,

the first explanations were attempted by the philosophers. Kant[1] marks an important epoch in sleep theories, since he definitely turns these problems over to science, declaring that physiologists alone are capable of formulating a satisfactory theory. To the earlier physiologists, explanations in terms of animal spirits, nervous fluid, cerebral vibrations, etc., were very easy. Sleep was commonly explained as due to the exhaustion of the nervous fluid or to a diminution in its mobility. During this period it was supposed to flow back to the brain and nervous centers. When the nervous system was sufficiently recharged and the recuperated muscles ready to respond, awakening resulted.[2]

With the advent of a more scientific physiology in the first quarter of the present century, vitalism was gradually forced to retire, but in sleep theories many of the elements that were closely associated with the older theories remained. Albrecht von Haller and Hartley, in particular, had insisted on the part that the circulation of the blood plays in the causation of sleep. Sleep was for them the result of pressure on the brain due to venous congestion, which impeded the flow of the nervous fluid or interfered with the cerebral vibrations. They also adduced many facts and observations in support of venous congestion. Subsequent investigators contributed still more, until quite an imposing structure was reared. Until 1870, this congestion theory in one form or another was that most widely accepted. The chief observations on which it rests are:

1. The resemblance of sleep to apoplexy, epileptic seizures, and allied pathological states, in which there is a clearly marked congestion.
2. Pressure on the brain produces unconsciousness in cases of fractured skull.
3. The horizontal position (and according to some authors, the head lower than trunk) is favorable to sleep.
4. Children and the aged (?) sleep more than the normal adult. This in the case of children was interpreted as due to compression of the skull upon the growing brain, in the aged, to loss of tone of cerebral vessels. For lack of compression, the sympathetic system never sleeps. Full blooded people are good sleepers.
5. Opiates and narcotics induce sleep by derivation of the blood to the head. (?)
6. 'All the causes which tend to produce sleep are causes that tend to lower the action of the heart or to retard the circulation in the brain' and hence to bring about a state of cerebral congestion. (?)[3]

In general criticism, it need only be said that this theory is now absolutely contradicted by experimental evidence, and

[1] Kant: p. 126.

[2] For historical résumés of sleep theories, see Duval (Jacoud), Ch. Pupin, Radestock (appendix). For the Vitalistic theories, see Hartley, Haller, Erasmus Darwin, Clutterbach, Hammond (1869), and Encl. Brit. (1799).

[3] Many of these observations are palpably incorrect.

serves as a warning of the danger of attempting an explanation of a physiological state by reference to a pathological state that bears to it only superficial resemblances.[1]

The next theory to gain general ground was still a theory in terms of blood circulation, but was directly opposed to the preceding, in that it attributed sleep to cerebral anæmia. Before attempting any general statement or criticism, we shall first treat of its history. Blumenbach,[2] in 1795, advocated the diminution of the circulation of the blood as the proximate cause of sleep. His conclusion was based upon an observation on the volume of the brain in a young man who had fractured his skull by a fall. The opening remained covered only by the membrane. During sleep the chasm was deep, in waking hours superficial. In 1821, a case of fractured skull in a Montpellier woman was reported,[3] and later another case by Kennedy, confirming Blumenbach's observations.[4] The first attempt at experimentation was made by Donders (1854), who cut away a piece of the skull of an animal and examined the condition of the cerebral vessels through a watch glass cemented over the hiatus. He discovered the condition to be that of anæmia. Kussmaul and Tenmer in Germany repeated the experiment. In 1855, Fleming reported that the compression of the carotid arteries on himself and other subjects produced a dreamlike state of consciousness, while compression of the jugular veins resulted in an entirely different state.

So far these experiments and observations were scattered and had not attracted general attention, yet together with the other objections that could be urged, they were sufficient, in many quarters, to shake confidence in the congestion theory. A few of the writers who treat sleep at all were favorably impressed with anæmia, while many others grew non-committal. The time was now ripe for the more thorough and convincing investigations of Durham and Hammond. As has frequently happened in the history of science, these experiments were carried on independently and almost at the same time. Durham was the first to publish. His monograph on the Physiology of Sleep marks the beginning of a new epoch in the study of sleep problems. It justly deserves to rank as a classic.

[1] With this theory can be associated such names as Cabanis, Bedford-Brown, G. H. Lewes, Metcalfe, H. Holland, Marshall Hall, Clutterbach, Sieveking, Dickson, Macnish, and W. B. Carpenter (in his earlier writings).

[2] Bluemenbach: (Tr.) p. 199.

[3] According to Durham the Montpellier case was reported by Caldwell, according to Hammond by Dendy. I have not been able to find the original.

[4] For other cases of fractured skull see Brown, Hammond (1869), Mosso (1880).

His experiments were performed chiefly on dogs. After chloroforming, a portion of the skull about the size of a shilling was removed by trephining from the parietal region and the dura mater cut away.[1] Durham found a remarkable contrast in the appearance of the brain during its periods of rest and activity. In the former state, veins, capillaries, and smaller arteries were indistinct, and the surface pale and below the level of the bone, while in the latter the vessels were filled with blood, clearly distinguishable in color, and the surface was distended and rosy. In order to satisfy himself still further as to the value of his experiments, he used the watch glass precaution for excluding the influence of atmospheric pressure, applied powerful lenses, and compared the cerebral condition of waking and sleeping dogs placed side by side. Later, by artificial ligaturing of carotids and jugulars, he confirmed Fleming's observations.

Rejecting the congestion theory, he attempts to formulate a theory of his own. His fundamental standpoint is seen in the definition of sleep as "that particular state of cerebral inactivity, which is essentially associated with nutrition and repair of brain substance." Now, according to Durham, cerebral inactivity is directly proportional to oxidation of brain substance. Consequently, whatever tends to lessen the mutual affinity of brain tissue and oxygen or to prevent that affinity from coming into active operation, may, within certain limits, become more or less directly the cause of sleep. Among such causes, he enumerates the diminished susceptibility of the brain matter itself, the diminished activity of the oxygen supplied, and the accumulation of decomposition-products which catalytically check the reaction. It is upon the last mentioned cause that Durham lays most stress. But none of these causes can come into decisive operation unless the requisite state of circulation can be established. The conditions of circulation usually obtaining during sleep are both peculiarly favorable and necessary to extensive nutrition.[2] The oxygen supply depends upon the state of cerebral circulation and this in turn is in a relation of direct mutual interdependence to the condition of the brain. Therefore, powerful predisposing causes to sleep may be found in whatever tends to decrease cerebral circulation. This decrease can be brought about either through the nervous system, *e. g.*, by presence or absence of stimuli,

[1] Durham: 1860, pp. 153 ff.

[2] During activity, the rapid and abundant flow of blood through the capillaries results in a large supply of oxygen, favors endosmosis into the vessels of products of oxidation, and is unfavorable to exosmosis of nutriment material from vessel to tissue. During sleep, the slower and diminished flow of blood favors exosmosis. *Vide* Kohlschütter (1869).

central or peripheral, or through the vascular system, *e. g.*, by decrease in force or frequency of heart beat.

Durham deserves much credit for the wide range of facts on which he bases his conclusions and for his scientific caution; but it is evident that his theory does not go far towards an explanation of sleep. It is fairly bristling with special problems, and is made up of elements that call for greater systematization and subordination. Traces of three more or less distinct lines, which investigators have subsequently followed, are visible: (1) the effects of auto-intoxication, (2) the necessity of oxygen to cerebral activity, and (3) the study of the mechanism by which the vascular changes are brought about. The first two tend to theories which emphasize chemical activities and reduce alterations in circulation to the role of a secondary factor. Hence we shall postpone consideration of the auto-intoxication, and a general discussion of the necessity of oxygen, until we come to treat of the chemical theories. An important point to notice about Durham's theory, and one that has been generally overlooked, is that he never definitely commits himself to more than the statement of a mutual interdependence of the condition of the brain and the condition of the circulation. He does not state whether cerebral anæmia is the cause or a result of cerebral inactivity.

Wm. Hammond's attention was first directed to the anæmic condition of the brain during sleep by a case of fractured skull in 1854.[1] His experiments (1860) were of the same character as Durham's, although not as extensive except upon the influence of anæsthetics and narcotics on the circulation. They yielded like results.[2] Later by means of a manometer he conclusively demonstrated that the blood pressure on the brain is less during sleep.[3] Hammond makes little attempt at theory. After pointing out the chief differences between sleep and stupor, he attributes the exciting cause of sleep to the necessity for nutrition and the removal of waste, the immediate cause to cerebral anæmia. This influence of anæmia is shown by the action of the heart, which draws the blood from the internal organs, including the brain, to the skin, thus causing sleepiness; by the action of extreme cold, which produces similar effects; by the action of digestion, which draws the blood to the alimentary tract; and by the tendency to sleep as exhibited in debility and excessive

[1] For Hammond's works, see Bibliog. at end.

[2] For other experiments, see Vizioli, Mosso (1880). Tarchanoff (1894) has recently by experiments on young puppies entirely eliminated the possible factor of chloroformization.

[3] Hammond (1869), appendix. Weir Mitchell's experiments confirm.

loss of blood. Although unelaborated, this is a clear statement of the vasomotor theory.[1]

Investigators of sleep phenomena now became more numerous. Their work, however, was still characterized by little community of labor. It will hardly be profitable to treat of all the investigations and all the theories of the vasomotor type in chronological order. We shall content ourselves, therefore, with picking out a few of the most typical.

Girondeau (1868), making use of the discovery by Boll and Robin of the lymphatic spaces around the cerebral vessels, attempted, in a rather fantastic fashion, to account for the retardation of the blood circulation by a compression of the vessels due to an accumulation of lymph. J. Leonard Corning (1882), in studying the connection of throbbing carotids with epileptic attacks, invented an instrument for carotid compression. Through its means, he found that a certain degree of compression in the evening was much more efficient in the production of sleep than in the morning. Arguing from this fact to the conclusion that the flow of oxygenated blood was in each case the same, he is led to seek the primary impulse to sleep in the exhaustion of stored intra-ganglionic material. The continuance of function depends upon the amount of this material. But there is another physiological factor, which, although following in time, is of equal importance with the first. The explosion of plastic material is immediately dependent upon the circulation which brings the oxygen supply. Other things equal, intensity of function is in the ratio of the blood supply. Corning thus correlates the two factors of exhaustion and anæmia.

A distinct advance towards a closer grappling with the problem can be seen in the work of C. A. Moore (1871). He attempts to account for the onset of sleep by a discharge of stimulating influence from certain ganglia of the sympathetic system in the neck along the vasomotor nerves of the cerebral arteries. This discharge occurs when the cerebro-spinal system, through fatigue of the brain, is no longer capable of inhibiting the sympathetic ganglia. Its effect is the reduction of the blood supply below the amount necessary to maintain functional activity, but not below the amount necessary for nutrition. Moore's theory is open to objections that make it at present untenable. Its refutation, however, is important in closing two possible lines of explanation that might be attempted. (1)[2] There is no such inherent antagonism between

[1] I shall employ the more usual term vasomotor, from the most typical in the group, in place of anæmia theory.

[2] See Porter: Donaldson. The experiments of Franck, 1894, p. 721, and of Goltz and Ewald, 1896, p. 389, are to the point.

the cerebro-spinal and sympathetic as Moore assumes; (2)[1] although high authorities have been led to believe in a special vasomotor system in the brain, the weight of scientific opinion at present is against any such system. But, however this problem may be settled, we have the strongest kind of experimental evidence that both the total amount of blood in the brain and the rapidity of flow depend chiefly upon conditions of general blood pressure.[2] Until that evidence is shaken, the probabilities are all in favor of a vasomotor theory that explains cerebral anæmia by a loss of tone in vasomotor centers. Such a theory is that of Howell (1897). It is the most recent and by far the strongest presentation of the vasomotor theory yet made. As such, it demands detailed criticism.

Howell's point of attack is gained from experiments performed on the dilatation of cutaneous vessels during sleep. A water plethysmograph was attached to the forearm of the sleeper, with the no-pressure point kept at the middle part of the portion immersed. Great difficulty was at first found in sleeping under such unusual conditions, and it was necessary, to gain satisfactory results, for the subject to fatigue himself by considerable out-of-door exercise or by light sleep on the previous night. The experimenter kept a record of the various conditions of the sleeper, and their effect on the curve. As soon as the subject lay in bed and attempted to go to sleep, the experiment began. The curve fell—the exact moment of the setting in of sleep was impossible to determine—until it reached its minimum in one to one and one-half hours, where it remained with minor oscillations for an hour or two. A quite gradual rise now commenced, growing more rapid for the half hour just preceding complete awakening. Besides the general course of the curve, there were two classes of oscillations noted: (1) short, brief, variable oscillations usually traceable to external stimuli or movements, (2) nearly hourly periodic waves probably due to rhythmic changes in the vasomotor center. Howell's experiments number twenty in all. Mosso has also made observations proving an increase in the volume of the lower limbs during sleep and a corresponding decrease during mental activity,—results which, before Howell, were confirmed by Bardeen and Nicols, and by Dr. Shields in the Johns Hopkins laboratory. Durham can be quoted in favor of dilatation of the skin vessels. He based his conclusion chiefly upon the increase of temperature at the surface, which can be seen even by the unscientific observer in the flushed condition of the skin, and upon the observations of Sanctonus, Keill and Edwards, which

[1] Foster, 5th Ed. Pt. III, pp. 1131 ff. Also, Pt. III, 1897. Mosso.
[2] Hill: 1896, pp. 40 ff.

establish that perspiration in sleep is at once more copious and solid than in the waking state. Innumerable observations in daily life go to show a reciprocal relation between the blood supply of the brain and skin. There can be no doubt of the fact of dilatation of cutaneous vessels during sleep. As to splanchnic dilatation, Howell, while admitting that it would perform the same function as cutaneous dilatation and would greatly aid in the production of cerebral anæmia, is inclined to deny it a place on the ground of lack of evidence. However, many of the highest authorities assign to the splanchnic vasomotor center the main role in the regulation of the cerebral circulation.[1] Hill's experiments show the absolute necessity, as well as the nicety of adjustment, of the splanchnic vasomotor mechanism for compensation of alterations in hydrostatic pressure due to changes of bodily position.[2] By paralysis or section of splanchnic vasoconstrictor nerves, death may follow from cerebral anæmia. The blood supply to the internal organs is not increased, for there is a noticeable decrease in function in all cases. Reciprocal relations of blood supply seem to hold here between skin and internal organs, as well as between skin and brain.

Taking the facts from his experiments as data, Howell proceeds to construct his sleep theory.[3] The derivation of blood to the skin vessels must necessarily result in a lessening of arterial pressure in the arteries at the base of the brain, thereby bringing about a condition of cerebral anæmia. If we seek the cause of this alteration of circulation, we must look to the vasomotor center. This, we know, is kept in a constant state of activity by the continual stream of in-coming and out-going impulses. That it is subject to fatigue can be shown by experiments upon the stimulation of the sciatic nerve in curarized and narcotized animals. Being thus placed at the very center of nervous activity, exposed to continual stimulation, a condition of fatigue, proportional to nervous activity, must necessarily result. Further, the long duration of sleep, after the cortical cells have regained their irritability sufficiently for arousal on moderate or slight stimulation, as shown by the curve of intensity of sleep (Kohlschütter, Michelson, and others), can best be explained by the necessity for longer rest of the vasomotor mechanism. The periodicity of sleep is, therefore, to be directly associated with the fatigue rhythm of the vasomotor cells. The lessening of the blood supply, while the immediate cause and under normal circumstances not only the necessary but the determining element, is not the only factor

[1] Foster: *op. cit.*; Porter: *op. cit.*
[2] Hill (1896): pp. 78 ff.
[3] In summarizing, I quote and abbreviate freely.

involved in the production of sleep. Two other factors, singly or together, may co-operate with anæmia: (1) a diminution of irritability, caused by fatigue of large portions of the cortical area (particularly sensory and association areas); and (2) voluntary withdrawal of sensory and mental stimuli, involved in preparation for sleep. For lack of experimental data, Howell refuses to add, as Durham and Corning did, chemical elements to his theory, or to discuss the exact relation of blood supply to functional activity or quiescence of the organ. His theory is by far the strongest presentation of the vasomotor theory yet made.

The main point of criticism is the importance attached by this theory to the fatigue of the vasomotor centers. There are two lines of attack. (1) The plethysmographic curve commenced to fall as soon as the subject assumed a horizontal position in bed and attempted to go to sleep. The curve for arterial pressure shows a similar fall under the same conditions. But these results need not be interpreted to be due wholly to loss of tone of the vasomotor cells of the bulb, for such a fall occurs whenever the subject assumes the conditions of rest. In Hill's experiments, the arterial pressure was as low in the waking state, when the subject lay in bed in the morning, as in the sleeping state in the evening. The horizontal position tends always to lower arterial pressure.[1] The absence of movement in the limbs also contributes to the same result, since it removes two important factors (the compression of the muscles and changes in position) in the return of the blood through the veins. These considerations emphasize the importance of purely mechanical and reflex causes in the circulatory changes of sleep. (2) A decrease in tone undoubtedly occurs in the vasomotor center,[2] but this loss of tone is by no means confined to that center. The other bulbar and spinal centers share it in common.[3] All reflexes are slow and less intensive.[4] At the periphery the decrease can be seen in the perceptions of dullness in the forehead and eye-lids, tingling in the conjunctiva, feeling of feebleness in the voluntary muscles, etc., which often precede sleep.[5] The nerves alone remain unaffected in their capacity for functioning.[6] The condition of the vasomotor center is, then, only one manifestation of a phenomenon, general throughout the nervous system. A search for the causes of this phenomenon leads to a general discussion of organic

[1] Hill: Lancet (1898), Jour. of Physiol. (1898). I can see no way of reconciling Hill's A. M. results with Howell's.
[2] Conclusively demonstrated by Patrizi, 1897.
[3] Manacéïne: pp. 20 ff.
[4] *Cf.* Lombard's knee-jerk experiments, 1887.
[5] Berger and Loewy.
[6] Bowditch (1890) has established the fact that nerves can function indefinitely.

rhythms, and of the relative importance of central and peripheral fatigue to the integrity of the nervous system. By the same evolutionary sanction that has given the nervous tissue control over the metabolism of all other tissues, the higher brain centers have gained domination over the periphery.[1] The cortical cells are chemically the most complex in the body.[2] The demands placed upon them for the maintenance of consciousness are heavy and irregular. While we may not exclude fatigue of the periphery as a possible factor, it is surely much more true to the known facts to look to fatigue of the higher centers and the necessity for their recuperation as the main determining element both in the time of occurrence and the duration of sleep. The initial loss of tone in the vasomotor center would then be chiefly traceable not to a local fatigue, but to Howell's two secondary factors, diminution in cortical irritability and withdrawal of stimuli. The divergence of the intensity curve of sleep and the plethysmographic curve is as readily explainable from this standpoint as from Howell's. The long continuance of sleep at low intensity would be due to the need for extensive storing of material within the cell to meet the demands of the day period, while the character of the plethysmographic curve would be accounted for by the continuance of the bodily conditions of rest, together with the low state of cortical activity and the lack of external stimuli.

From the time of Durham, circulation theories have taken as their foundation cerebral anæmia. There is another class of theories, however, which, while recognizing the force of the new experiments, has managed to save some of the elements of the older congestion theory. Sergouyef,[3] influenced largely by the definition of sleep as a state in which active nutrition is maintained without diminution, accepted anæmia of the cortex, but believed in hyperæmia of the portions at the base of the brain. His position is somewhat strengthened by Brown-Sequard, who reports observations of hyperæmia at the base of the brain in many animals during sleep, and has also shown that congestion can be produced in the same way as cerebral anæmia by bilateral section of the two great sympathetics. De Boeck and Verhoogen have more recently attempted a somewhat similar theory. According to them, the resistance to the flow of blood in the arteries supplying the cortex is greater than in those supplying the base of the brain, since they are smaller, longer and more tortuous, while the other arteries, shorter and more numerous, leave the Circle of Willis

[1] *Cf.* Verworn, p. 572.
[2] Fish: p. 109.
[3] See Berger and Loewy.

at right angles. For this reason, when the arterial pressure is lowered, anæmia in the cortex is relatively greater than in the basal ganglia. This, as Howell points out,[1] would serve to explain how functional activity of the cortex could be suspended upon moderate fall of pressure, while that of the lower centers remains. There is, however, danger in over-estimating this difference, for we have the best of reasons for believing that the activities of nutrition are not absolutely heightened, but only relatively, in proportion to tissue waste. We have shown above that the loss of tone in the nervous system is general. Owing to his peculiar mechanics of circulation, Cappie, originally a full fledged congestionist, is still able to maintain venous pressure upon the brain substance as the immediate cause of sleep.[2] He accepts the Monro-Kellie doctrine that the total quantity of blood in the cranium, since it is a closed cavity, is invariable, and that only changes in its distribution can occur.[3] Hence, arterial and capillary anæmia is to be correlated with venous congestion. Granting that the Monro-Kellie hypothesis is true, still the compression of the brain substance could not occur: for under physiological conditions the intracranial pressure (the pressure of the brain upon the cranium) and the cerebral venous pressure are the same. The brain substance transmits pressure. It, itself, is incompressible.[4]

Now that we have examined the most typical, we are ready for a summary of the principal evidence upon which the vasomotor theories rest, and a consideration of the adequacy of any such theory to cover the facts. (1) Observations made upon the condition of the brain in cases of fractured skull, show a decrease in brain volume during sleep, and mental inactivity, an increase during periods of activity and excitement. This has been confirmed by experiments with the manometer. (2) Observations upon the condition of the cerebral blood vessels, after artificially exposing with all due caution portions of the brain surface, have uniformly shown an anæmia in arteries and capillaries. (3) Experiments in carotid compression, when not carried beyond certain limits, have produced states most similar to sleep, while jugular compression has resulted in a comatose state, utterly dissimilar. (4) In cases of fracture at the base of the skull or of traumatic injury to the cribiform plate of the ethnoid bone, permitting the escape

[1] Howell: 1897, p. 340.
[2] For Cappie's works, see Bibliog.
[3] Hill, 1896, p. 30, without asserting absolute invariability, replies to two arguments that have commonly been urged against the Monroe-Kellie theory.
[4] Demonstrated by Grashey. Hill: *op. cit.*, p. 2.

of the cerebro-spinal fluid,[1] the escape is found to be either absent or diminished during sleep and mental quiescence. (5) The retina is genetically an outgrowth of the forebrain. An examination of the retinal vessels by the ophthalmoscope shows them to be in a contracted state during sleep, while awakening is accompanied by dilatation.[2] (6) By various experiments, it has been shown that many of these areas, whose blood supply stands in reciprocal relations to the brain, are congested during sleep, or that a derivation of blood to them produces somnolence. (7) The arterial pressure decreases during sleep. The pulse rate is slackened. The frequency and force of the heart beat are lowered. (8) One of the chief causes of insomnia is a disturbance of the vasomotor mechanism. It is a general law, which, of course, may be cut across by other special laws, that whatever tends to increase the cerebral circulation, increases cerebral activity and is hostile to sleep.[3]

These facts are all well attested and are sufficient to put beyond any reasonable doubt the anæmic condition of the cortex,[4] notwithstanding the immense complexity of conditions of cerebral circulation and our present ignorance concerning them. Many writers, like Vulpian or more recently Berger and Loewy, without committing themselves to congestion, have urged objections against cerebral anæmia. These objections are all founded upon insufficient observations or insufficient knowledge of the experimental work of others. No sleep theory that pretends to anything like completeness can afford to slur over these facts or attempt to pass them by without explanation. The great point of controversy, in the past, has been as to whether anæmia is the cause or the result of the physiological condition of the nervous system during sleep. This question is not answered by mere appeal to the fact as established by manometer, sphygmograph, and plethysmograph records, that vascular changes occur prior to the onset of sleep. So also do changes in respiration, and in muscular tone. A search for the cause of sleep has led in our own case, and we believe must necessarily lead, beyond the circulatory system and the particular nervous elements directly connected with it, to alterations occurring in the higher nervous centers,—the histological modifications of the cortex

[1] Thomson, Hill and Halliburton, 1899.

[2] Hughlings-Jackson: 1863.

[3] For a summary of experiments and observations upon insomnia, see Manacéïne, Chap. II. This law is founded upon a wide factual basis.

[4] We leave the question of cerebral venous anæmia open. The question can have no bearing on the anæmia theories, for venous congestion, if it occurs, can only be regarded as a compensation for circulatory changes, never in the sense of the older congestion theories as a cause of sleep.

and the deeper lying chemical changes that occasion them. The shutting down of the circulation is only one in a cycle of events. It results from a temporary derangement of the nervous mechanism, and in turn adds still further to the disintegration. As to the importance of anæmia as a secondary factor in the causation of sleep in man, it is yet too early to make a just estimation. In this connection, it should be remembered that the absolute quantity of blood in the brain and cord is unquestionably small: to judge from observations upon animals, it is not more than one-tenth of the entire blood in the body.[1] Such a statement, however, needs to be qualified by the consideration that it is only the cell-bodies which are well supplied with blood. We have also seen that the nerve cells are, in a certain measure, independent of the circulation. Whether or not vasomotor changes are an essential factor is a question to be left to comparative physiology.

## II. The Chemical Theories.

The vasomotor theories, with which we have been dealing in the preceding paragraphs, were developed chiefly by English physiologists. Shortly after the important experiments of Durham and Hammond, researches in physiological chemistry reached a stage in development sufficient to serve as a basis for a new line of sleep theories. The principal exponents of these chemical theories are to be found among French and German writers. Naturally, their period of greatest influence did not come until after that of the anæmia theories. Largely as a matter of convenience in treatment, although not without some scientific justification, we shall divide the chemical theories into two groups: (1) the combustion theories; (2) the auto-intoxication theories.

### (1). The Combustion Theories.

The common trait of the theories, which we shall treat under this rubric, is that they are all theories in terms of oxygen or carbonic acid gas. The most typical gives the name to the group. Historically, they belong to the earlier period.

In 1867, Pettenkoffer and Voit performed the classical experiments which resulted in the gaining of a new point of departure. These investigators found that the amount of oxygen absorbed during the night was greater than during the day, while the amount of carbonic acid gas eliminated was considerably less.[2] Sommer, following a year later and inter-

[1] Donaldson: Am. Text Bk., p. 736.

[2] Serious doubts are thrown upon the validity of the results of Pettenkoffer and Voit, for their tables show not only that the absorption of oxygen during sleep is greater than the elimination of carbonic

preting these results, attempts to show that sleep results from the exhaustion of reserve oxygen, which has been stored up in the cells and blood during the night for the activities of the day.

Pflüger (1875) elaborates the theory still further, and lays the greatest stress upon a gaseous exchange.[1] The stored up intramolecular oxygen is replaced by carbonic acid gas. When the replacement is complete, all the activity ceases. The intramolecular conditions which accompany the formation of carbonic acid gas are such that the atoms of its molecules at the moment of its production are in a state of most violent oscillation, just as in explosion. These explosions, which are constantly taking place throughout life, excite vibrations in the surrounding atoms, and radiate far and wide along the lines of connection of the nervous system and its annexes, causing the explosion of other cells. The molecular changes are attended by a consumption of oxygen greater than living molecules can take up at the same time. Hence, the amount of carbonic acid gas produced must necessarily diminish. Now the cortex is probably the seat of more active chemical processes than any other portion of the body, and therefore requires a large oxygen supply. As the production of carbonic acid gas diminishes, the explosions become less numerous and the condition of relative cerebral inactivity—sleep—results. The analogy of the vibrations of the strings of the harp, which continue vibrating for a long period after the blow which set them in motion, gives us a clue for the explanation of the long duration of sleep after the reabsorption of oxygen in the brain has commenced. The loss of irritability during sleep is due to the exhausted condition of the nerves and the greater cohesion of cerebral molecules, caused by diminution of the total heat produced by chemical processes in the brain. Sleep or waking depends primarily not upon the amount of potential energy in the brain, but on the amount of *vis viva* of the intramolecular movements.

The theory of Pflüger is by far the most completely developed of all this group. Various other writers have emphasized the part played by carbonic acid gas in the causation of sleep, some attributing to it a positive sedative effect;[2] but the importance

acid gas, but that it is greater than the absorption of oxygen during the waking period. The safest view is that during sleep as during hibernation there is a diminution in both the production of carbonic acid gas and the absorption of oxygen, but that the diminution in the production of carbonic acid gas is relatively greater. See Saint-Martin, Vol. CV, pp. 1124 ff. Experiments of Quinquaud and Zunts. Am. Text Bk., p. 546. Manacéïne, pp. 8, 171.

[1] I abstract freely from Lancet.

[2] See Liv. Age, Vol. XCVIII, 1868, pp. 213 ff; Wurtz.

of carbonic acid gas as a positive factor has been left in the background by the line of development which the auto-intoxication theories have taken.

There is abundant evidence to prove the presence of a reserve supply of oxygen in the cells. An amœba continues movement in an atmosphere of hydrogen for twenty-four minutes; an excised muscle remains irritable for even a longer period.[1] In both cases after complete exhaustion oxygen is necessary to restore vitality.

The question of whether or not the cortex is the seat of very active combustion cannot, at present, be said to be definitely settled. Results from Mosso's[2] temperature experiments and Hill and Nabarro's[3] experiments on gaseous exchange in muscles and brain are contradictory. Pflüger is, without doubt, right in attributing to the cells of the nervous system the greatest sensitivity to the lack of oxygen. This would explain the quick and violent death of the higher vertebrates from asphyxia, in comparison with the slower death of lower organisms, since in them the nervous system has gained a more complete control of the respiratory and cardiac muscles. The other cells, in particular the ciliary cells, continue to function long after the nerve cells have ceased.[4] Speck (1892) has performed an important experiment, directly to the point. He inhaled air from a vessel, counting at the same time. The air lost its oxygen. When the oxygen of the inhaled air fell to eight per cent. of an atmosphere in pressure, the counting stopped and unconsciousness resulted, although the other functions of the body showed no alteration. Recent investigations have brought out fundamental facts concerning tissue oxidation, and have exhibited at once the great complexity of the chemical processes occurring in the cell and our immense ignorance of them. "The amount of oxidation is not increased in an atmosphere of pure oxygen nor within wide limits is it affected by variations in atmospheric pressure."[5] A certain amount only of oxygen is needed, and this amount is determined by the amount of metabolism, not the reverse as was formerly supposed.[6] As to the cause of cell decomposition, we have evidence that certain bacteria, chiefly in the intestinal tract, cause putrefaction, but as to the main cause we can only say that it is internal, in the cell itself, and resembles fermentation. The function of oxygen appears, then,

[1] Verworn: p. 263.
[2] Mosso, 1894, finds the temperature of the brain frequently higher than that of the aortic blood.
[3] Hill: 1896, pp. 152 ff.; Hill and Nabarro, 1895, find combustion of muscles very much greater than of brain.
[4] Verworn: *loc. cit.*
[5] Lusk: Am. Text Bk., p. 945.
[6] See Loeb, 1899, p. 165.

to be simply the oxidation of injurious decomposition products. With our present meager knowledge it would be hazardous to attempt any definite statement in regard to the correlation of mental activity with the amount of oxidation in the cortex, and entirely unwarranted to attribute sleep to such a simple factor as the lack of oxygen (Sommer)[1] or decrease in gaseous exchange (Pflüger). However Pflüger's conception of sleep or waking as dependent upon the *vis viva* of chemical processes, provided *vis viva* is interpreted to mean both a certain intensity and extensity of cortical activity, is a profitable one.

### (2). The Auto-Intoxication Theories.

The common starting point of auto-intoxication theories is the influence of certain products of decomposition of living substance upon the continuance of cell activity. These products act not merely catalytically, but, as the name auto-intoxication implies, have a decided and positive effect upon metabolism.

Obersteiner (1872) was the first important advocate of the theory that the cause of sleep is to be found in the accumulation of acid products in the brain. Previously, the acidity of the cortex in fatigue and sleep had been noted. Obersteiner laid especial emphasis upon lactic acid. The condition of the circulation, although secondary, he admitted to be of great importance, since upon it depended the removal of the fatigue products.

Preyer (1877), through the weight of his name, gave wider currency to the theory.[2] He first distinguishes sleep from allied pathological states by the fact that normal sleep is always preceded by some fatigue of muscle, sense organ, or brain. The condition of mental activity is a certain amount of oxygen. "Now as the brain of a sleeping animal receives as much blood as it does when the animal is awake, we are compelled to infer that the influence of oxygen on the waking and sleeping brain is different." This must be due to the accumulation in the blood of such fatigue products as lactic acid and creatine. A reduction of the amount of oxygen available for cell activity results from the great affinity of these substances for oxygen. We have, then, three well defined and causally connected stages: (1) fatigue due to the accumulation of certain decomposition products; (2) sleep, an intermediate state; (3) awakening, resulting from the complete oxidation of the fatiguing substances.

[1] Foster has pointed out that on the exhaustion theory it is necessary to explain why the respiratory centers are not stimulated to greater activity, as a compensation, by anæmia. Foster, 1891, Part IV, p. 415.

[2] See Preyer, Med. Examiner, 1877.

The auto-intoxication theory cannot be held in the form stated by Preyer, for it rests upon two assumptions: (1) that the presence of oxygen is the essential condition of mental activity, and (2) that the circulation remains unaltered during sleep.

Rachel (1883) offers a theory in many respects more completely worked out in detail than Preyer's, and somewhat less objectionable. He uses facts from experiments on muscle fatigue to show that activity may cease from the action of fatiguing substances, notwithstanding the presence of material available for metabolism. From the partial independence of the cell from the blood supply, he contends that the purifying function of the blood is of more importance than its oxygen-bearing function. Sleep results from the fact that the capacity of the blood supply for elimination of fatigue products is limited. The direct cause is a surplus of such products, accumulated from all portions of the body. Secondary factors are found in a diminution in amount of intramolecular oxygen and in a decrease of intravascular pressure, not necessarily followed by a pronounced anæmia in the capillaries. Rachel differs from Preyer in holding that the fatiguing substances are already oxidized, and does not attempt to trace just how they affect cell metabolism.

The more recent auto-intoxication theories show clearly the influence of the development of the science of bacteriology and the investigations made upon animal poisons. The epoch making work of Selmi on cadaveric poisons did not appear until 1870. He gave the name of ptomaines to the transition products in putrefaction. They are basic in character, and are formed by the action of bacteria upon organic matter. Brieger restricts the term ptomaine to the non-poisonous basic products, while he called the poisonous products toxins.[1] In distinction to the ptomaines are the leucomaines, also basic in character, but the results of cell decomposition, without the action of bacteria.

It is the fatiguing and narcotic action of many of these leucomaines that serves as the point of departure for the sleep theory of Errera. These substances are formed more rapidly than they can be oxidized, and being carried along by the blood stream, are retained by the cerebral centers in sufficient quantity to produce sleep. During sleep the leucomaines are oxidized, and the oxidation products, having no special affinity for the cortical cells, are carried away by the blood. Hence, for Errera as for Preyer, sleep is only one stage in a more or less rhythmic cycle. Insomnia due to excessive fatigue is produced by the differing effect of the larger quantity of toxic substance.

Berger and Loewy (1899) have lately argued to the same effect.

[1] Brieger: Ueber Ptomaine, Berlin, 1885–86.

They, however, have seen the necessity of completing the chemical explanation by a histological one, and have refused to limit the action of the narcotic substances to any one portion of the nervous system.

We are now ready to discuss in detail the more important evidence upon which the auto-intoxication theories rest.

Preyer made many attempts to produce fatigue by the injection of lactic acid, with very variable results.[1] Ranke was more successful in the use of a solution of beef broth with a greater variety of fatiguing substances.[2] Mosso, by the injection of the blood of a fatigued dog into the circulation of an active one, was able to induce complete symptoms of fatigue.[3] Mental derangement and cases of mental disorders during infectious diseases show a close correspondence between the psychological state and the presence of poisonous and convulsive substances in the body.[4] Perhaps, the most important evidence of all comes from the experiments on the toxicity of the urine, first exhaustively studied by Bouchard. Bouchard's experiments were performed by collecting the urine of three different periods of the day (7.15 A. M.–3.15 P. M.; 3.15–11.15 P. M.; 11.15 P. M.–7.15 A. M.).[5] After neutralizing and filtering, the fluid was injected into the veins of the animal. He found that, although the urines of sleep were more rich in solid matter, in equal volumes they were almost always less toxic than the urines of the day.[6] The difference between the two urines is further brought out by the fact that the one is antagonistic to the other. The total uritoxy of a mixture of equal quantities of both urines is less than the sum of their uritoxies.[7] By the help of charcoal acting on the urine so as to separate its constituents, seven distinct toxic principles were found:[8] (1) a diuretic substance—urea; (2) a saleogenous substance; (3) one that contracts the pupils; (4) one that lowers the temperature; (5) an organic convulsive substance; (6) a mineral convulsive substance—potassium; (7) a narcotic substance. This narcotic substance was fixed, organic, not fixed by carbon, soluble in alcohol, and found in alcoholic extract with urea and other substances. This substance has not yet been chemically analyzed, and hence it is named only from its physiological effect. In sleep, the urines are always markedly convulsive, while those of the day period are very little or not at all convulsive, but produce narcosis. In a passing remark concerning the auto-intoxication of sleep, Bouchard suggests that sleep may be due to the accumulation of these narcotic substances, while awakening is due to that of the convulsive substances.[9] Experiment upon repose after great muscular exertion in the open air shows a diminution in toxicity by nearly one-third, both during the period of exercise and that of rest. Bouchard concludes that the low toxicity is the result of the more complete oxida-

[1] Preyer: *op. cit.*
[2] Ranke: Archiv. f. Anat. u. Physiol., 1863, p. 422; 1864, pp. 320 ff.
[3] See Donaldson, Am. Text Bk., p. 740.
[4] See Oliver, in Introduction to Bouchard.
[5] Bouchard: pp. 36 ff.
[6] The ratio of 7:5:3 held for the different periods.
[7] The uritoxy is the unit of measurement, first employed by Bouchard. It is the toxic amount necessary to kill a kg. of living substance.
[8] Bouchard: p. 262.
[9] Bouchard: p. 41.

tion of organic substances during muscular activity. This might be used to explain the disturbance of sleep, following excessive fatigue, since the organic narcotic substances would be decreased through oxidation, while the mineral convulsive substances would not be affected. Investigations upon the toxicity of the blood have even a more direct bearing on sleep problems than those upon urine. Bouchard's estimate from his own experiments is that one kilogram of blood can kill between 1,250–3,000 grams of living tissue.[1] This, he admits, is perhaps too high an estimate. However, death in an animal results from increase in the amount of poison in the blood to two and one-half times its normal quantity.

We have already pointed out the danger of interpreting a physiological by reference to a pathological condition. Arguments from the effects of drugs and narcotics are precarious, so also arguments from Bouchard's experiments, for, in all, urine was injected until death of the animal followed. The mental state produced prior to death was that of stupor, not sleep. Yet, inasmuch as toxic substances are shown to result from functional activity, there is a strong probability that under normal bodily conditions their accumulation plays a part in the causation of sleep. They may act physically, *e.g.*, by mere clogging, as well as chemically.[2] To assume that auto-intoxication is the sole cause of sleep, or to attribute the narcotic effect to any one group of decomposition products, is to dogmatize. "We really know only that living substance is continually undergoing decomposition, for this is apparent from the output of decomposition products. But as to the path from the complex proteid compounds to the end products, as to the special chemical transformations that take place, our knowledge is very incomplete, since, as yet, the composition of proteids is known very slightly."[3] On the auto-intoxication theory, notwithstanding Errera's attempt, it is extremely difficult to explain the long duration of sleep.[4] An insufficient supply of nutrition, in the end, leads to the same results as an accumulation of decomposition products. In all normal fatigue curves the loss of power is at first rapid and then slow.[5] These facts suggest that at the beginning of sleep the more active factor is an auto-intoxication and that, when the oxidation process of the fatiguing substances is completed, the intensity of sleep sinks and remains for so long a period at low level, in order to allow the cells to recover from the effects of exhaustion of the day and to lay in a store of new materials. Thus, exhaustion as well as an accumulation of fatigue products would precede sleep. Both would be causally connected with its onset. The exact relation between the two, of

[1] Bouchard: pp. 69 ff.
[2] Loeb: p. 165, 1899.
[3] Verworn: p. 161.
[4] *Cf.* Howell, p. 338.
[5] Donaldson: 1897, p. 312; p. 322.

course, needs working out, but there is no valid reason for singling out, as the auto-intoxication theories do, only one of the two factors that produce fatigue, to the exclusion of the other. Concerning the relative importance to sleep (in man) of the decomposition products accumulating in the nervous tissue from inadequate removal and those that are imbibed from the blood stream we are also in the dark. There is no way at present of determining. The direct accumulation of waste products, checking the activity of the cells that produce them, is the more fundamental of the two, since it occurs in the absence of blood circulation.

The chief criticism of all the various chemical theories considered we have found to be a lack of adequate experimental basis. What are we to say of the possibility of a chemical explanation of sleep? It is the aim of physiology to bring vital phenomena under the same physical and chemical laws that have been established in the inorganic world.[1] Although physiological chemistry is tremendously hampered at the outset by the fact that an analysis of living tissue is impossible, there is no sufficient reason for thus early giving up in despair or returning to vitalism. Progress must necessarily be slow and labored. We have unequivocal evidence to show that the chemical composition of the fatigued nerve cell,[2] whether the fatigue be produced by artificial stimulation or as the natural result of a period of activity, is different from that of the rested cell. If the ultimate explanation of the phenomenon of sleep is to be found anywhere, it must be sought in the chemical composition and the chemical changes occurring in the nerve-cell. The work so far done gives us hope that, if not a complete explanation, at any rate a more complete than any yet given may be forthcoming. In the meantime it is the part of wisdom to avoid any too hasty or too ambitious theorizing that will tend to limit the scope of investigation.

One objection that has frequently been urged against the chemical theories so far has never been satisfactorily answered. Monotonous repetition of a stimulus or reduction in the number of adequate stimuli may produce sleep without the presence of fatigue. We shall reserve a consideration of this objection until we come to treat of sleep from the evolutionary standpoint. From any other point of view a full answer is impossible.

## III. The Histological Theories.

The great improvement in microscopical apparatus and technique, together with the discovery of new histological methods,

[1] *Cf.* Verworn, Chap. I.
[2] See Chap. III.

within the last decade has opened up a new and attractive field to science. The outlines of the nerve-cell and its processes, brought out with clearness, show the neurocyte [1] to be a structural unit. With the recognition of the morphological independence of the nervous elements, one of the main problems for the nerve histologist is to account for physiological interdependence. Hence, many painstaking investigations have been made upon the difference in condition of the neurocyte during functional activity and during repose. These investigations have a direct bearing upon the problem of sleep, and have resulted in the formulation of numerous sleep theories.

The first of these theories to deserve attention is that of Rabl Rückardt (1890). The conception that underlies his theory is that the neurocytes possess a kind of amœboid movement, which allows them to make or break contact by means of a retraction or expansion of the prolongations of their neuro-dendrons. Sleep and hypnosis are the psychological correlates of a partial paralysis of these amœboid prolongations, resulting in an isolation of nervous elements. Lepine (1894), in attempting an explanation of anæsthesia and motor paralysis in hysteria, is led to a somewhat similar theory of sleep. He confines the amœboid movement to the extremities of the neurodendrons. Querton may also be quoted in support. Duval's theory (1895) is by far the most widely known and elaborated of any of these theories. Wiedersheim's alleged direct observations of amœboid movement in the neurocytes of the superior ganglion of Leptodera hyalina have largely influenced him towards amœboidism. He does not regard actual contact as essential for transmission of nervous impulse, but only a certain degree of contiguity. During sleep, this degree of contiguity is lost by a retraction of the ramifications of the cell-processes. He explains the functional activity of the lower centers during sleep, by the fact that currents of contact are set up among the amœboid prolongations by chemical processes started by decomposition products (chemiotropism). Chemiotropism is not intense enough to allow the impulse to reach the cortex. When the necessary degree of chemiotropism is reached and the currents of contact have become sufficiently extensive, awakening results.[2]

There remain but two other histological theories to be considered. Since they are both largely the result of negative criticism, it might be well here to attempt a brief summary of the most important facts discovered concerning the neurocyte in functional activity, quiescence, and fatigue.

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[1] Terminology of Fish.

[2] Pupin (1896) strongly defends Duval.

1. As to cell-body.

Flesch (1884) noted the difference in the reaction of cells to staining reagents after activity, and distinguished cells on this basis as chromophile and chromophobe. Vas (1892) experimented upon the cervical ganglion, and confirms Flesch. Vas found that the immediate result of activity was a swelling of the cell. Mann (1894) extended the observations to motor cells of the cord and sensory cells of the retina of the dog. He concludes that during rest there is a storing of several chromatic substances, which are used up in activity. In the cells observed there was in normal activity an increase in the size of cell, nuclei and nucleoli. Fatigue was characterized by a shrinking of the nucleus and probably of the cell and by the formation of a different chromatic substance. Lugaro (1895) in the main confirms Mann. The decrease in volume has been put beyond doubt by the thoroughgoing investigations of Hodge (1892-94). He found the most clearly marked differences in the nuclei, the decrease depending upon the length of time of stimulation. The outlines of the cell and nucleus became irregular, and vacuolation was present. Hodge also demonstrated the presence of fatigued cells in various animals after a day of normal activity. Further experiments made by the same investigator upon the effects of old age showed likewise a decrease in the volume of the cells and nuclei and an increase in pigmentation.[1] Heger (1899) found a shrinking of cell-body in ether, chloroform, and morphine narcosis.

2. As to cell processes and appendages (gemmulæ).

Heger has shown that in animals decapitated in the waking state, under normal conditions, the neurocyte possesses numerous dendrites of uniform calibre throughout their whole length, provided with many gemmulæ. The appearance differs somewhat from one species to another, but remains the same in animals of the same species. Under the action of chloroform, morphine, alcohol, and prolonged electric stimulation, Demoor (1899) noted the loss of gemmulæ and the formation of moniliform varicosities, which disappeared on return to normal state. Experiments of Stefanowska, Querton and Heger, confirm the observation. The results of Lugaro's experiments in the main agree.[2] He is convinced from his own and Ramon y Cajal's experiments that many of the varicosities observed are attributable to defective technique. The formation of these varicosities he regards as occurring independently of the retraction of gemmulæ, and as an indication of fatigue. He found little alteration in the condition of the trunks and larger branches. Berkeley[3] has shown that the gemmulæ are irregular and absent in certain mental disorders. After numerous experiments upon normal, over-excited, hibernating, narcotized animals, and animals killed by cold, Heger[4] concludes that variability is an important property of the neurocyte, and that these variations can occur in the cell-body, the prolongations, or the gemmulæ, simultaneously or independently. Hodge[5] has extended these investigations to normal fatigue, with similar results. During the growth of the brain, while medullation of fibres has not taken place extensively in the cortex, mass movements of cells to gain their position in the adult structure

[1] Hodge: Jour. of Physiol., 1894.
[2] Lugaro: 1898.
[3] See also Obsteiner, Die nervösen Centralorgane, Leipzig, 1888, pp. 112 ff.
[4] Heger: *op. cit.*
[5] Hodge: Amer. Jour. of Physiol., II, No. 3, p. 13.

doubtless can occur. Notwithstanding the seeming positiveness of these results, the question as to whether or not any movement occurs in the developed neurocyte at all comparable to the movements of the amœba or leucocyte must be regarded as still open, so long as Kölliker and Ramon y Cajal refuse to give their assent.

Ramon y Cajal's refusal to accept amœboidism is due to his belief in the great mobility of the neuroglia cells, in which the neurocytes are bedded for support. This forms a starting point for a theory of sleep.[1] The neuroglia cell by an intervention of its pseudopodia between the neurocytes, he supposes, can block or stop the passage of nervous impulses, so that various degrees of nervous coherence are possible. Experiments have confirmed the mobility of the neuroglia cells.

Lugaro (1898), accepting the plasticity of the gemmulæ, presents an entirely original theory. According to him, in normal mental activity only a few of the possible connections between neurocytes can at a given time come into play. It is necessary that the rest be interrupted by a retraction of gemmulæ in order to impede access of other stimuli, which would be able to deviate or suppress the one that is being elaborated in the cell. Strong stimuli or those of immediate importance to the well-being of the organism can, of course, break through. With the exhaustion of the contractility of the gemmulæ due to fatigue (auto-intoxication in particular), greater torpor of their movements results. Consequently, contacts are multiplied until the nervous impulses become more and more diffused throughout the cortex and individual processes are lost in the maze. The same condition can be reached through a lack of stimuli. Lugaro repeatedly appeals to psychology to support his theory, particularly to the phenomenon of attention. Wundt's[2] is by far the most satisfactory psychophysical theory of attention. It possesses innumerable advantages over Lugaro's. As an explanation of the cortical state during sleep and dreams, Lugaro's theory is seen at its best. But dreams are only one among many abnormal, or better non-normal mental states, which are characterized by a disaggregation of the normal consciousness into smaller and more elementary groups.[3] Dreams give the type of the greatest dissociation. If psychological facts are to be urged in behalf of histological theories, their weight would seem to be in favor of that of Duval or Ramon y Cajal. A compromise between Duval's and Lugaro's theory is not, however, an impossibility.

It is hardly within the province of a layman to attempt de-

[1] Ramon y Cajal: 1895.

[2] Wundt: 1887, Bd. I, pp. 282 ff; G. E. Müller's theory is also well known, Zur Theorie der sinnlichen Aufmerksamkeit, 1873.

[3] *Cf.* Janet, L'Automatisme psychologique, 1894, pp. 484 ff.

tailed criticism of histological theories. From a glance at those presented, it will be seen that three lines of attack, and the only three lines possible, have been taken: (1) a dissociation of nervous elements, caused by an amœboid movement of the attachments of the neurocyte; (2) a dissociation of nervous elements, caused by an interposition of the pseudopodia of other cells; (3) a diffuse connection of nervous elements caused by torpor of movement of the attachments of the neurocyte. The decision between these three must be left to future investigations of the histologist. It is no reflection on the importance of such research to admit that the histological theories can only be theories of the changes occurring in the nervous mechanism. The causes of its derangement will still have to be sought.

## IV. The Evolutionary Standpoint.

In our consideration of the various lines of sleep theories we have found that no one of them alone is adequate as a complete explanation. The vasomotor, the chemical, the histological theories are all capable of being based upon a wide range of facts. Their several claims cannot be ignored, but must be reconciled. In the immense complexity of the human organism, the primary factors of sleep are so overlaid and obscured by secondary ones that it is next to impossible to trace them out. Hence the necessity, if profitable work is to be done in the future by the physiologist, for a broader point of view. The sleep problem must be attacked genetically. It is true that comparative physiology is still in its infancy, that at present few facts are known concerning sleep phenomena except in the higher vertebrates. To attempt any elaborate theorizing would be useless. There are, however, enough facts to point unequivocally to the conclusion that sleep is the product of evolution, and to indicate the main lines which any evolutionary theory will have to follow. Three questions, which cannot be kept wholly separate, naturally arise.

1. What is fundamental in the phenomena?
2. How has selection operated upon sleep? How have the secondary factors crept in, and what is their relative importance?
3. How shall we define sleep? What are the limits of the problem?

Hodge and Aikins (1895) have made a careful study of the daily life of a protozoan, with special reference to the rhythm of rest and activity. Observations were recorded upon a vorticella, for a period of twenty-one hours, without interruption. Thirteen other experiments were afterwards made. Automatic contractions of cilia were continuous, those of the vesicle

nearly so, while contractions of the stalk were irregular and occurred usually upon presence of stimuli. The conclusion to which the writers are led is that vorticellæ neither rest nor sleep. Hodge assumes that the unicells possess a rudimentary form of consciousness. Whether or not the lower organisms do now or have once possessed consciousness, which has since dropped out, is a disputed question. It would be unwise, before there is a more general consensus of opinion among authorities, to encumber a sleep theory at the outset by definite commitment to any theory of the origin of consciousness. These experiments with the vorticellæ are of great value, since they show clearly, what might be expected *a priori*, that in a simple organism with adequate provision for food supply and removal of waste products continuous response to stimuli without diminution of irritability is possible. There is no reason to hold that fatigue is a general phenomenon common to all life.

For the sake of making the problem of the fundamental in sleep more definite, let us try to picture what would be the mental state of the simplest organism capable of serving as the support of consciousness, and living under conditions so favorable that fatigue could not occur. This primitive consciousness we may, without violence, suppose to be a motor consciousness. If we exclude the action of hostile agencies in the environment, the only occasion for the cessation of consciousness would be the lack of adequate stimuli. A state of full consciousness[1] would exist in the presence of an adequate stimulus, and on its absence a mental blank or a vague organic sensation. At irregular intervals states of consciousness and unconsciousness would succeed each other. The ultimate reason for the presence of the latter is not to be sought outside of the organism in the withdrawal of stimuli, for stimuli are always present, but in the internal conditions which render the organism unable to receive or respond. The chemical composition of the cells is relatively too simple, the nervous elements too poorly organized, or the vascular system, if blood circulation be present, defective. The operation of these causes is not confined to the lower animals. Strümpell[2] reports a case of general anæsthesia in a boy of sixteen, who was left with the sensibility of only one ear and one eye unimpaired. Sleep could be produced at any time by the closure of the eyelid and the stoppage of the ear. Through an inherent limitation of capacity, other than that manifested in or brought about by fatigue, the physiological processes underlying consciousness may sink to so low a level of intensity and extensity that

[1] Idea or sensation given in a state of attention and felt. *Cf.* Titchener, Outline of Psychology, New York, 1899, pp. 249 ff.

[2] Strümpell: Pflüger's Archiv, Bd. XV, p. 573; Nature, Dec. 13, 1877 (tr.).

sleep results. This inherent limitation of capacity is to be conceived of as due to a state of development inadequate to meet the demands of continuous consciousness. We shall include all the foregoing causes as well as those that spring from degeneration under the rubric Lack of Development.

The factor, Fatigue, now demands our attention. Even without the presence of consciousness, the development of the multicellular organism must have continued to the point where a fatigue of nervous elements would occur, for, with increasing complexity of structure and function, the utility of bringing the cells and groups of cells into more intimate relations with each other, in order to gain unified co-operation and advantageous adaptation to environment, becomes apparent. The performance of this function calls for a developed nervous system. The ascendency of nervous tissue over all other tissues is to be correlated with an increase in chemical complexity of the cell and an increased metabolism. For its demand upon food supply and for removal of waste, the nerve cell is left dependent upon the alimentary tissues and the circulatory system. Fatigue, implying among other things a diminution of irritability, thus inevitably arises in the course of development. If such a condition would be brought about without the presence of consciousness, its onset is made all the more certain and rapid by the possession of mentality, for consciousness once present has a survival value and adds greatly to the demands made upon the organism. Were it not for fatigue, the development of the nervous system might be carried to such a point that consciousness could be present continuously. Fatigue is the one great factor that forever makes sleep a vital necessity. Arising in the course of development, it ends by limiting the possibilities of development. Operating as a cause it limits further capacity for function, though it, itself, results from a limitation of capacity. As such it can be placed along with the other causes under the rubric Lack of Development. But Fatigue is a positive, well defined, wide spread condition arising only after exercise of function. It can be clearly distinguished from the other conditions inhering in the organism that limit capacity; and since the distinction is an important one and we have no other term to use, we shall confine the term Lack of Development to those causes other than fatigue.

Sleep, then, results from the limited capacity of the organism to receive and respond to stimuli, either through Fatigue or through Lack of Development. Both factors are internal. The relation of each to function can be traced along chemical, histological and vasomotor lines. What is their relative importance in the causation of sleep? In man, fatigue by itself can occasion sleep, while a reduction of external stimuli to the

minimum is not sufficient to cause sleep in an individual of highly organized and well nourished brain without the presence of fatigue; for central excitations, along with the remaining peripheral, would maintain an adequate support for consciousness. In animals low down in the scale, cessation of consciousness can occur without the presence of fatigue, by the mere withdrawal of adequate stimuli. Thus at one extreme of the series fatigue alone can cause sleep, at the other lack of development. Between the extremes the two factors do not stand in relations of reciprocal importance, for in most instances the more poorly organized and developed the nervous system, the more easily is it fatigued. This is well illustrated by the ease with which children, savages, and the insane are fatigued by work that requires mental concentration, and their great need for sleep in comparison with the normal adult. On the other hand, the short sleep periods of great men have often been remarked.

We are now ready for our second question as to the operation of selection upon sleep and the rise of the secondary factors. In every organism at all developed, we find firmly rooted habits of activity and repose. These habits are rhythmic, and, if not ultimately caused, have been powerfully influenced by rhythmic changes in environment. Even without the presence of fatigue, such changes would have been capable of establishing rhythmic periods of greater and lesser activity; but, fatigue being an inevitable product of evolution, it becomes all the more imperative that rest periods be grouped. It is an obvious economy to the organism that the rest periods should correspond to periods of lessened adequate stimuli. A very profitable field is open to the physiologist in tracing out this correlation. Loeb has already shown the importance of light to animal life. Heliotropism,[1] quite comparable to that in plants, is observable in the animal kingdom. The influence of light upon sleep habits[2] is clearly brought out by the fact that animals which depend chiefly upon smell and hearing are much less regular than those with largely developed vision, such for instance as birds. The former can sleep quite independently of the time of day, while sleep in the latter corresponds closely to the rhythm of daylight and darkness, even varying with the changes of season. Lack of data compels us to leave the most fascinating problem of sleep rhythms in this fragmentary way.

With the development of the organism and the appearance of new adaptations, an opportunity for the operation of new internal factors is afforded. The most important of these is the blood supply. That the circulation is not fundamental can

[1] Loeb: 1890.
[2] Donaldson: 1897, p. 297.

be seen from the fact that in insects and plants, devoid of a vascular system, well defined periods of rest and activity occur.[1] When the organs of blood circulation appear, according to the principle of utility their development is accompanied by an increased control of the nerve cells over the supply. A complex and nicely adjusted vasomotor mechanism is the result. The rhythm of this system is derived from the rhythm of the vasomotor nerve cells. Their rhythm in turn is not independent of, but directly determined by, the primary rhythm in the central nervous system, so that while the higher centers are made dependent upon the circulation for food and removal of waste, they are dependent upon it as a master upon the performance of the work of a servant, not as a servant upon the bounty of a master. The precise work that the circulation performs in bringing about the condition of sleep in man is probably rightly estimated by Howell,[2] who urges that diminution of blood supply can best explain the sudden and comparatively simultaneous onset of sleep.

High up in the animal series, where consciousness plays an important role, another factor enters,—conscious adaptation to the conditions most favorable to sleep. A glance at the arrangements of modern sleeping rooms, the preparations usually made before retiring, and the bodily position assumed for the night's repose, will show that they have as their objects the reduction of external stimuli to a minimum and the derivation of blood from the head. Adaptations originally conscious may, of course, become reflex and appear later as inherited reflexes.[3]

There remains but one other factor to be considered. The psychophysical phenomenon of attention cannot be overlooked.[4] The hypnotist in artificially producing sleep resorts to a sudden stimulus or repetition of monotonous stimulus, frequently a command or suggestion, capable of holding the attention. A complete sleep theory should seek to trace the factors exhibited in the artificial state as they occur in the natural state, and to show their relative importance to sleep phenomena in general. Hypnotic sleep is most easily explained physiologically by inhibition. Certain processes on their arousal inhibit other processes in the cortex, reducing them below the level of extensity required for consciousness. It is possible that fatigue, produced by excessive activity of portions of the cortex,

[1] Loeb: 1898.
[2] Howell: 1897.
[3] In this connection Goltz' experiments are of interest. His decerebrized dog "slept" like any normal dog. Many of the habitual positions in sleep might well be purely reflex; see Goltz, Der Hund ohne Grosshirn, Pflüger's Archiv, Bd. LI, 1892.
[4] *Cf.* Külpe, Outlines of Psychology (Tr.), 1895, pp. 452 ff.

may also contribute powerfully to the result. The influence of repetition of monotonous stimuli upon sleep has often been attributed wholly to fatigue, but in our judgment it more properly falls under the same general explanation as hypnotic sleep. We must deny to these causes fundamental importance to the production of sleep, for their operation is accidental and without universal significance. The nervous mechanism is not so poorly constructed that under normal circumstances it is obliged to suspend its chief function from an antagonism of parts.

In a work that is admirable for its array of facts, Madame Manacéïne has attempted to state a theory of sleep in the brief formula: "Sleep is the resting period of Consciousness."[1] The phrase is not intended as a metaphor, but is put forward seriously as an explanation and continually used as such. There is probably no field in science, where the loose use of terms is more harmful than in psychophysics. The formula as it stands is meaningless nonsense. Consciousness, as such, has no more need for rest than it has for nutrition. It is true that the sleep problem has a psychological as well as a physiological side. Investigations of hypnagogic states and of dreams are to be correlated with investigations of the condition of the brain. Between the two series, no causal relation can be assumed. But just as the *raison d'être* of death is to be sought in a breaking down of the physical organism, so the *raison d'être* of sleep is to be sought in a temporary derangement of the nervous system. Manacéïne's formula, when put into scientific language and properly interpreted, is a profitable conception. Sleep is the resting period of the *support* of consciousness. This differentiates the phenomenon of sleep from all other rhythms, and gives the sleep problem a definite place under the more general problem of rhythms in the nervous system. The term *sleep* is then reserved for that particular phase of the rhythm of the cortical or nerve cells which has as its psychological accompaniment a cessation of consciousness. It is well that such a differentiation should be sharply made. The nyctitropic movements in plants, the rhythm of activity and repose in a decerebrized dog or (if consciousness be denied to them) in the invertebrates, are only comparable to sleep on its physiological side. Without the psychological accompaniment, the term is no more applicable here than it would be to all periods of loss of power in the diurnal fatigue rhythm.[2] A word perhaps needs to be said concerning the various states that resemble sleep. In some cases it may be difficult to determine; but as the conditions of sleep become more fully known, it will become

[1] Manacéïne: pp. 59 ff.

[2] For curve of diurnal rhythm, see Lombard, Jour. of Physiol., 1892; Ostanikow and Gran, Neurolog. Centralbl., 1893.

easier to distinguish between the normal and the abnormal. Little light upon the sleep problem can be expected from a study of abnormal states. Such a study demands first a thorough understanding of the normal, and is rather a field for the application of general principles already discovered to special problems, than a source of new knowledge.

The three questions raised at the outset have been but meagerly and imperfectly answered. It is enough to have pointed out some of the difficulties and the possibilities that lie before an evolutionary theory of sleep.[1]

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Manacéïne at the end of Chap. I has an excellent bibliography. She also gives a short account of many sleep theories (pp. 40 ff), although with little attempt at criticism. I take pleasure in acknowledging my indebtedness to her work for much of the purely historical part of this article.


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[1] Besides the various theories we have treated, there are local theories of sleep; see Manacéïne, pp. 40 ff., and purely psychological (and mystic) theories. Their defects are so palpable that, with limited space, we have not thought it worth while to consider them. For typical theories, see Binns, Bigelow.

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# A 'COLOR ILLUSION.'[1]

By M. F. McClure, B. S.

In "Studies from the Yale Psychological Laboratory," Volume VI, 1898, there is an article entitled "A Color Illusion" by Professor George Trumbull Ladd. In it reference is made to a colored diagram, called an "example of Stilling's Charts," used for testing color blindness. The chart consists of a pale green background, 36 mm. by 44 mm., which is divided into squares of 1.8 mm., by white lines 0.4 mm. in width. On this background a red letter E, 21 mm. by 34 mm. is constructed out of similar red squares. It was noticed that when this figure was observed with a fixed gaze for a few seconds, some or all of the squares disappeared, and were replaced by green squares like those of the background.

When it was found that the illusion was not dependent upon the white division lines, a series of experiments was devised by Dr. E. W. Scripture to test the effects of varying the color of the background—or "substituting color"—while retaining the same red as the "disappearing color," without dividing either background or strip into squares. A set of Milton-Bradley colored papers was used, and out of these, sheets, 20 cm. by 30 cm., were cut for backgrounds, while small strips, 1 cm. by 20 cm., were cut, ready to lay on the backgrounds. To make the experiment, a background of a desired color was fastened to a board by tacks, and a colored strip was fixed on it by a pin; the whole was then observed at a distance of about 3 m. Although the results varied somewhat with different observers, yet "they remained fairly constant of the same order." The results obtained by a standard red strip, fixated on various backgrounds, may be divided, says the author, into two classes. With certain backgrounds—green, blue-green, violet, blue and black—the illusion of disappearance and substitution takes place quickly and suddenly. With other backgrounds—yellow, orange, light gray, white, light blue, light green and a light reddish violet—the substitution takes place with great difficulty, or not at all. If the same experiment is made with an orange strip, the illusion comes with green, dark violet, dark blue or black blackgrounds, only after persistent trying. On the red

[1] From the Psychological Laboratory of Cornell University.

strip the color darkens, but on the orange strip it lightens, and the background color seems to encroach on the strip from both sides until (where the strip does not disappear entirely) it is but a narrow "line of sunlight."

By way of general explanation, fatigue, in the sense of the Young-Helmholtz theory, is suggested. The colored strip is said to fatigue the eye and thus to create a temporary blind spot, which is filled in by the color of the background as the permanent blind spot is filled from surrounding retinal areas. The explanation is admitted to be unsatisfactory, for (1) dark strips seldom disappear on light grounds, and (2) orange grew brighter and lighter, instead of darker, before disappearing.

Aside from the general difficulties which the principle of fatigue or exhaustion encounters (the rise of negative after-images after a very short exposure, while the eye is, nevertheless, able to function throughout the day), there are here two special difficulties. (1) Why should a limited retinal area succumb while other areas, which are similarly exposed to stimulation, are unaffected? In the second place, (2) fatigue, as generally conceived, does not produce an entire absence of visual sensations, but only a change. Exhaustion to red, for example, produces some other color than red, depending on conditions of stimulation. The exhaustion of the "red-fibre" does not straightway throw the visual apparatus out of function; it only changes the reaction of the apparatus.

Since the explanation is avowedly inadequate, and since the phenomena described have considerable interest in connection with theories of color-vision, at large, it has seemed worth while to repeat the Yale experiments with some modifications.

The experiments which are described below were carried out in the Cornell Laboratory during the academic year 1899-1900.

Sheets of colored paper (Milton-Bradley), 10 cm. by 15 cm., were cut out for backgrounds, and strips, 0.5 cm. by 10 cm., were cut out to be placed upon these backgrounds. A pin with a small, round, black head fastened the strip vertically to the middle of the background, and also fastened the background to a black upright screen, which was placed 1.5 m. from the observer. An uniform light fell on the strip, background and screen from behind the observer, so that no shadows or reflections were produced. A series of eighteen colors were used as backgrounds, while only the six 'standard' colors were used for strips. After a preparatory "Ready," the signal "Now" was given, a stop-watch was started, and the observer fixated the head of the pin. If the strip disappeared, and complete substitution took place, the observer indicated the fact, and the time of disappearance was noted. If it did not disappear, the fixation was continued for two and a half minutes. The intro-

spection was then written, and the eyes were rested until all after-images had disappeared, before proceeding with the next combination; the time allowed for rest being at least three and a half minutes. The colors used were:

Violet red (V. R.). Rather dull and dark.
Red (R.). Dark and saturated.
Orange red (O. R.). Vivid, brighter than R., saturated.
Red orange (R. O.). Brighter than red, very saturated.
Orange (O.). Very bright and vivid, strong.
Yellow orange (Y. O.). Bright, unsaturated.
Orange yellow (O. Y.). Bright, darker than yellow, unsaturated.
Yellow (Y.). Bright, very little saturated.
Green yellow (G. Y.). Vivid, bright, thin.
Yellow green (Y. G.). Bright, well saturated.
Green (G.). Moderately bright, not very saturated.
Blue green (B. G.). Bright, not very saturated.
Green blue (G. B.). Rather dark, not very saturated.
Blue (B.). Well saturated, darker than green blue.
Violet blue (V. B.). Dark, well saturated.
Blue violet (B. V.). Dark, moderately saturated.
Violet (V.). Rather light, not highly saturated.
Red violet (R. V.). Rather light, not very saturated.

The observers were Dr. Lane (L.), Miss Winger (W.), Miss L. Hempstead (H.), and the writer (M.), all students in the Psychological Department.

The first fact investigated was the disappearance of the strip. Out of 408 tests—each strip with all the different backgrounds repeated with the four observers (6x17x4)—there were 93 'disappearances.' Of these, 37 were reported by H, and the other 56 were distributed almost equally among the other observers (19, 20, 17). The number of disappearances of the strips, running through the spectrum from red to violet, was as follows: 14, 18, 8, 13, 23, 17; blue disappearing most often and yellow least often. There is a good deal of variability in the backgrounds upon which the strips were lost. Only once did all four observers report a disappearance with the same combination of strip and background. This was with *B.* on *V. B.* There were 10 concurrent disappearances with 3 observers: *V.* on *G. B.*, *B. V.* and *R. V.*; *B.* on *V. R.*; *G.* on *R. O.*; *O.* on *R. O.*, *Y. G.* and *G.*; *R.* on *V. R.* and *B. V.* Some disappearances occurred with every background: *R.*, *O. Y.* and *Y.* gave the fewest (2 each), and *R. O.* the most (10). When one remembers the heterogeneous nature of the papers, variations in brightness, color tone and saturation, and mixture of pigments, one can hardly wonder at the apparent confusion in the results. It is worth noting, however, that in the 11 concurrences which we have mentioned there exists in each case a likeness between the strip and the ground either in (1) color tone, (2) brightness, (3) lack of saturation or (4) in two or three of these moments at the same time.

Much more important than these numerical results (considering the unsatisfactory character of the estimate) is the introspective account of the experiments. Introspection brought out the following facts.

A. At the first glance the contrast, both in color and brightness, was greatest. This contrast was at times very striking.

B. Quite soon, however, the strip and ground began to lose saturation, and to approximate each other in brightness, until, in cases of complete disappearance, the brightness of the strip and of the ground were the same. Although both colors yielded to each other more or less, the strip yielded the most and was merged in the ground.

C. After the approximation in brightness, but before its disappearance, the strip was covered by a 'veil' of the background color, through which the color and form of the strip could be seen in a greater or less degree. Sometimes the color of the strip spread over the background to a small extent; the veiling of the strip occurred, however, in almost every case.

D. The strip then lost its identity, going entirely into the background which had diminished in saturation and in brightness of tone.

Beside this general course in the visual sensations, a number of special points were noted. (1) In 82 tests the strip either approximated the color of the background, or became merely a line of brightness with definite or indefinite outlines.

(2) In 44 tests the strip narrowed down to a small area directly around the pin head. At times, both color and form remained here; again, only color; and still again, just brightness remained.

(3) In 54 combinations the one end disappeared entirely, almost to the center. At times the one end would go and then the other, but never both at the same time, the upper end reappearing before the lower one went and *vice versa*.

(4) The background was surrounded at times by a halo of the complementary color.

(5) In 22 cases, the background or the strip lost in saturation, and approximated the other in brightness; it became a "peculiar, luminous gray;" and, finally, changing over, acquired either the color complementary to the original color of (a) the background or of (b) the strip, or (c) some other color.

Again, (6), in some instances, the strip and background simply lost saturation, and became grayer.

## CONCLUSIONS.

Since the invocation of the principle of exhaustion or fatigue has been found inadequate, we propose to interpret the facts which have now been collected in the light of a rival theory

of visual sensations,—the theory of Hering. It is very evident that at least some of the phenomena which we have called attention to are intimately connected with Local Adaptation and Contrast. Suppose that we take Hering's account of these two groups of facts, as it is given in the "Zur Lehre vom Lichtsinne," and attempt to apply his explanations to the results before us.[1]

First in regard to our general results.

A. The increase in color and brightness differences at the very first is a matter of simultaneous contrast (pp. 42 and 129).[2] This is due to the indirect effects of stimulation.

B. The approximation in brightness and the decrease in saturation is due to *local adaptation*. As the effect of adaptation, all brightnesses tend toward a medium gray, and all colors tend toward their complementaries, passing through gray, the point of minimal saturation (pp. 131-134).

C. The veil of the background color which spreads over the strip is caused by simultaneous light induction.

This is, again, an illustration of the fact that the retina is an unit and functions as a whole. As a consequence, the effect of stimulation is not limited to the region directly affected but extends to neighboring portions of the organs (indirect stimulation). It is to be noted that the effect of simultaneous induction is reciprocal; the color of the strip spreads to the ground, and that of the ground to the strip (pp. 19, 29, 129).

D. This is simply a later stage in the processes already described. Adaptation continues to affect the colors of the ground and of the strip. There seems, however, to be a slower adaptation for the more extensive ground (we know that the color of a small patch is very unstable; *cf.* pp. 131-2) and hence a preponderance in the successive color induction from this to the strip. A very important point in this connection is the fact that the background is only *relatively* stable; it undergoes change as surely as does the smaller strip. This fact gives the key to the whole phenomenon. It seems to have been overlooked by Professor Ladd. To test the effect of steady fixation upon the background, the whole series of "substituting colors" was used without the strips, *i. e.*, they were simply fixated upon the black screen. Out of the 72 tests (18x4), 19 gave gray (total loss of saturation), 41 gave a partial loss of saturation, 8 gave the complementary color and 4 some other color. *In no single case did the ground retain its original quality.* The sudden "vanishing" of the strip, which occurs at times, is probably

[1] A good general account of the phenomena of Contrast and Adaptation and their explanation may be found in H. Ebbinghaus, *Grundzüge der Psychologie*, I, 1897, pp. 217-229, 245-263.

[2] The references are to the *Lichtsinn*.

due to fluctuation of attention at the point where differences of the strip and ground are almost eliminated.

The more special facts related above do not call for extended explanation. The cases in (1) are made intelligible by the smaller area of the strip as compared with the area of the background. Here extension is a factor in the determination of the qualitative effect, as in temperature it influences quantity (intensity). The instability of a small color-stimulus is more easily demonstrable if a liminal extent of color is taken. The exact value of the extensive attribute of visual sensations needs working out. The peculiarities of (2) and (3) are doubtless to be referred to attention to the black fixation point, and to differences in distribution of the visual substances. The halo in (4) is another instance of simultaneous induction (p. 129). We have already considered the conditions operating in (5). For the physiological processes in this case see pp. 126 ff.

It is worth noting that whenever the strip became complementarily colored it was a yellow or a violet strip,—a poorly saturated stimulus,—one, therefore, that stands near gray, and hence near its complementary. Where some color other than the complementary followed the loss of saturation, it was the color which would have been produced by the mixture of the strip color, or of its complementary, with the induced color from the ground; *B.* on *Y. O.* gave *G. Y.* three different times (*B.*+*Y. O.*=*G. Y.*),[1] and *V.* on *O. Y.* gave *Y.* (comp. of *V.*=*G. Y.*, *G. Y.*+*O. Y.*=*Y.*). Again, in the case of the grounds, it was the yellows and the violet which went over to the complementary color; both with the strip-background experiments and with the backgrounds by themselves. All these things we should expect to follow local adaptation and induction. Point (6) represents the first stage of adaptation.

Our observers noted the apparent lightening of the bright colors, orange, yellow and green, and darkening of the red and blue, with decreased saturation. A similar fact was mentioned in the Yale experiments. We are very strongly inclined to regard this as a misinterpretation of the facts. The absolute brightness of colors is difficult to determine. We underestimate the large brightness differences in the spectrum. These differences come out sharply when the color-tone is reduced or eliminated through the process of adaptation. A red is so rich, so saturated, so "taking," that we do not realize its small brightness valence; hence, when it begins to grow gray, it seems gradually to darken. One can easily convince oneself of this fact, by fixating monocularly for some time a good red, meanwhile

[1] Blue was a saturated color, and evidently did not quite reach the gray stage.

looking for an instant, from time to time, at the same stimulus with the unused eye alone. In this way one can bring the dulled and the original red, practically, side by side and easily compare them in brightness as well as in saturation. This method will also quickly bring the conviction that the phenomenon here in question is not a 'strip-phenomenon,' but rather that it embraces the whole field of visual sensation. The strip is no more truly affected than the background, and the background than its surroundings; its changes are more striking because it happens to be smaller, and because it lies in the focus of the attention. The phenomenon is no more an 'illusion' than is any change in visual sensation which is a result of the temporal course of stimulation.

# THE PERCEPTION OF VISUAL FORM.[1]

By L. Hempstead, Ph. B.

The object of the present Study is to record the perceptions set up by simple visual forms shown to the observer upon a background subliminally or liminally different from themselves. We wished to ascertain whether the illusory perception and associative completion of figures, exhibited under these conditions, obeyed any general law; whether constant individual differences could be made out, as between observer and observer; and whether the development of typical figures, as they passed through the stage of subliminal to that of liminal and thence to that of supraliminal difference from their background, followed any constant course. The problem proved to be more complex than we had supposed, and the results here published are only preliminary.

## § 1. Method and Apparatus.

The experiments were performed during the academic year 1899–1900. The apparatus used was that devised for another purpose by Leuba,[2] and modified by Whipple.[3]

The apparatus consisted (*a*) of a blackened tube, 50 cm. in length and 4 cm. in diameter. At a distance of 8 cm. before this stood (*b*) a rotating disc, 49 cm. in diameter, in which were cut 12 radial slits (20 cm. long, 10 mm. wide towards the periphery and 15 mm. wide towards the center of the disc) extending to a distance of 2 cm. from the outer edge of the disc. The disc was made of middle-weight junk-board, faced with gray paper. The electric motor which carried the disc was slung between upright wooden guides, in which it had a vertical movement of some 15 cm. The tube pointed through the upper portion (above the axis) of the disc: so that the lower the disc, the greater was the amount of gray interposed between tube and object. At a distance of 150 cm. from the disc was placed (*c*) a standard, carrying a gray card, 15.5 cm. square, upon which was pasted an outline figure of gray (about 7 cm. square, and 8 mm. in width of line). Only outline figures were used, as the limen for solid figures proved to be a good deal higher than that for outlines.[4] The gray of the background was the Milton-Bradley Co.'s neutral gray, no. 1; that of disc and figure was the slightly lighter warm gray, no. 1. The work was done in the dark room, and the necessary illumination produced (*d*) by two

[1] From the Psychological Laboratory of Cornell University.

[2] *Am. Jour. of Psych.*, V, 376.

[3] *Am. Jour. of Psych.*, IX, 569.

[4] This is evidently due to the multiplication of 'lines of difference' in the outline figures. *Cf.* the difficulty with which the place of junction of several lines is perceived in the experiments cited below.

incandescent lamps in the regular 110 v. alternating current circuit. The first lamp, 16 c. p., hung 20 cm. in front of the disc; the second 32 c. p. (ground glass) hung 35 cm. in front of the object-card. Both were shaded on the side towards the observer. The illumination was not absolutely constant from day to day, though it was sensibly constant during an experimental sitting. A large sheet of black cardboard, fitted to the observing tube, cut off the diffused light of the lamps and screened the whole apparatus from the observer.

The method of work was as follows. The disc was placed at its lowest point, so that the difference between figure and background was subliminal. At a "Now!" following a warning "Ready!" the observer looked through tube and disc, fixating the gray card. Five sec. were allowed for observation; the introspective record was then written down. After the disc had been raised 5 mm., the experiment was repeated; and so on. The time required for a single experiment (inclusive of that taken by the observer to draw and describe what he had seen) was 5 to 10 minutes.

We had anticipated that there would be a considerable error of expectation; but the anticipation was not borne out, either by the objective results or by the introspective records. It was so difficult to make out the figure at all, that the observer seemed to surrender himself wholly to the present impression, rather than to look for a modified form of the preceding impression. It need hardly be said that the observers never saw the figures, save under the conditions of the experiment, and that nothing was said to them as to the rightness or wrongness of their interpretations.

The figures employed numbered 71; and these were turned, some through 90° and some through 180°, so that the total number of stimuli was 142. A few were employed but once; the more suggestive were used much oftener, though not so often as to be recognized by the observer. After a long period of practice 300 experiments were taken. In each of these the disc was raised 8 to 30 times, and each of them gave from 5 to 30 different form perceptions.

The observers were seven in number.

The observers were as follows: (1) Miss L. Aldrich, *A*, a psychologically trained observer, alert, and objective in type; (2) Miss J. Cochran, *C*, a trained observer of the subjective type; (3) Miss L. Hempstead, *H*, a partially trained observer, of the objective type, working with full knowledge of the experiment and its aim; (4) Dr. W. B. Lane, *L*, a trained psychologist, of an extremely subjective type, possessed of some general knowledge of the nature of the experiment; (5) Miss M. F. McClure, *M*, a trained observer,—very alert and imaginative, but still of the receptive or objective type; (6) Miss I. G. Robertson, *R*, an untrained observer, objective; and (7) Miss F. M. Winger, *W*, a trained psychologist, with some knowledge of the experiment, not so easily classed as the preceding observers, but perhaps rather subjective than objective in her attitude towards the experiment. In

order of exactness of perception, the observers fall into 5 groups: *A*,—*R*,—*H*, *M*,—*W*, *L*,—*C*.

It does not appear that the type of the observer, or his general attitude towards the experiment, exerted any marked influence upon the experimental results. All that can be said is that the observers who knew nothing of the experiment, and were alert and objective in temperament, showed a somewhat slighter tendency to get a regular progressive series of illusory perceptions, and a much slighter tendency to see figures that bore no resemblance to the stimuli, than did the observers who were more suspicious and subjectively minded, and who knew or guessed something of the character of the experiment. Certain well defined tendencies of interpretation may be traced through all the records.

## § 2. The Various Forms of Illusory Perception.

The results of the experiment, under this heading, may be classified as follows.

A. *Perceptions which Bear a Definite Resemblance to the Stimulus.* Through all the hundreds of examples of these perceptions certain general and unifying principles may be clearly traced. They are:

I. For the more objectively grounded perceptions:

(1) The joining of different parts of a figure, *i. e.*, the formation of a continuous from a discontinuous figure. This joining may be:

(*a*) Of several isolated portions of a figure. This is irregular and rare, occurring most often where the figure is but feebly sensed. It may be the condition of several of the illusions given under B below. See Plate I, Fig. 1 *a a'*.[1]

(*b*) Of opposite end-points of a given line, or of a terminal point and a straight line. This joining occurs very frequently when the added line runs in a direction similar to that of lines of the figure: otherwise it is rare. Pl. I, Figs. 2 *a a'*, 3 *a a'*.

(*c*) Occasionally, of two lines whose junction can be effected by the continuation of a line in a direction parallel to itself. Pl. I, 4 *a a'*.

(*d*) See 2 *b* below.

The joining lines themselves may be:

(i) straight, so that the connection will be accomplished in the shortest possible way. Pl. I, 5 *a a'*.

(ii) a continuation of a given line,—hence taking its direction. Pl. I, 6 *a a'*.

(iii) curved, as if from mere indefiniteness of direction. Pl. I, 1 *b b'*.

[1] The figures in the columns under the simple letters are the figures used as stimuli; those under the same letters accented show the interpretation put upon these figures by the observer.

(iv) of a form, the 'cue' to which is taken from the lines of the figure. Pl. I, 2 *b b'*.

(2) The continuation of lines. The lines of the given figure may be continued:

(*a*) Indefinitely and irregularly, when parts of the figure first begin to be sensed. Pl. I, 3 *b b'*.

(*b*) When by so doing two portions of the figure will be joined to form a more unitary whole. Pl. I, 4 *b b'*.

(*c*) When by the continuation a symmetrical figure will be produced. Pl. I, 5 *b b'*.

(*d*) When the continuation is in a direction similar to that of other lines of the figure. Pl. I, 6 *b b'*.

(3) The rounding of angles. When the angles of a figure first begin to be perceived, they appear round. This may possibly be an example of the joining of two portions of a figure, made possible by the higher liminal value of solid figures: the angles being rather solid than open forms. Very often the impression of roundness persists even after the angularity has been perceived. Pl. I, 1 *c c'*.

(4) The failure to perceive lines. It often happens that complete lines of a figure are not sensed, although the observer is confident that he has seen the whole form, and is ready to conclude the experiment. This tendency is especially marked when the lines are:

(*a*) Short and disconnected. Pl. I, 2 *c c'*.

(*b*) Included lines. Pl. I, 3 *c c'*.

II. For the more subjectively grounded perceptions:

(1) Tendencies affecting the number of lines.

(*a*) The addition of lines.

(i) What seem to be purely subjective lines appear, to a greater or less degree, with all observers, and are included in nearly all outline figures. They occur throughout the whole experiment, though seldom towards the end. They are very fleeting and uncertain. It is unusual for them to persist for more than two observations, or to be at all constant whether for different observers with the same figure or for the same observer and the same figure at different times. The only—and that a dimly defined—underlying principle is that of symmetry. Pl. I, 4 *c c'*.

(ii) Lines which more definitely conform to rule are added when they can be joined to the ends of given lines and run parallel to other lines. Pl. I, 5 *c c'*.

(*b*) Uncertainty in regard to number of lines. Great uncertainty is shown as to the number of lines, especially in those figures in which a definite form or direction of line is repeated. Pl. I, 6 *c c'*, 1 *d d'*.

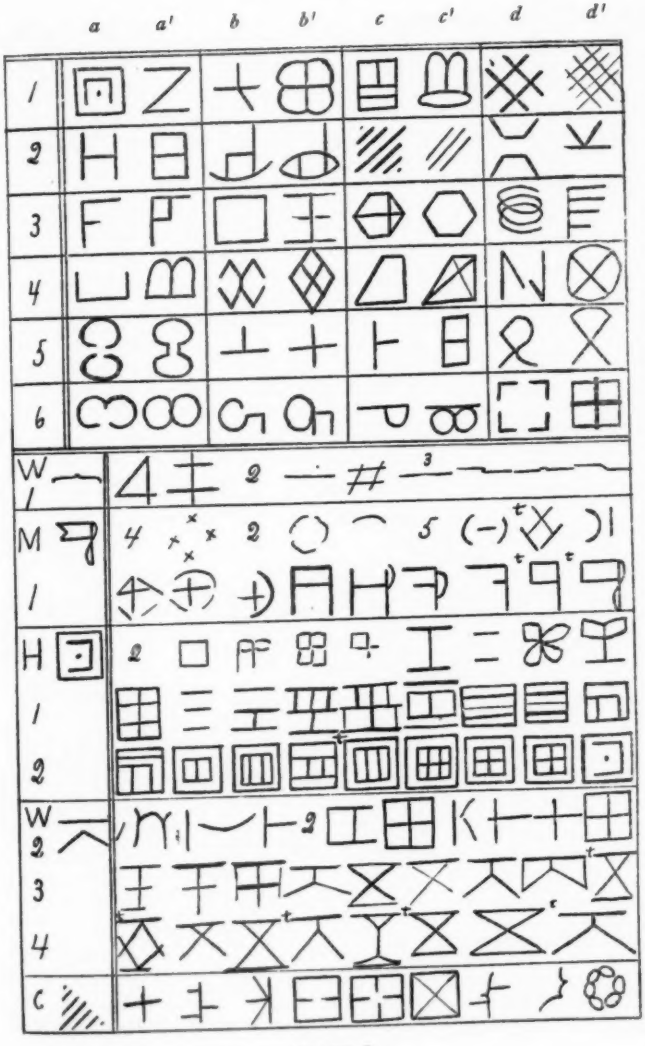



PLATE I.

(2) Those relating to the position of lines. When the figure is as yet very faint, great indefiniteness in respect to position is shown. Pl. I, 2 *d d'*.

(3) Those relating to the form of lines.

(*a*) Lines which are nearly straight are apt to seem straight. Even curved lines, when short or but slightly curved, occasionally appear straight at the beginning of an experiment. Pl. I, 3 *d d'*.

(*b*) Very occasionally, feebly sensed straight lines appear curved. Pl. I, 4 *d d'*.

(*c*) Very often the 'cue' to form is taken from a part of the figure, and applied from this to the whole. Pl. I, 5 *d d'*.

III. Individual Illusions. But one example of an illusion characteristic of only one observer was found. In figures which had a broken outline, *R* saw dark lines crossing the background and cutting the outline where incomplete. Pl. I, 6 *d d'*.

These illusions do not occur singly, but in all manner of combinations, the illusory lines themselves sometimes furnishing a basis for further illusions.

B. *Perceptions which Bear no Traceable Resemblance to the Given Figure.* Toward the beginning of the experiments, and often for some time during their progress, all sorts of figures were seen, which bore no apparent resemblance to the given figure (Pl. I, *M*, *W* 2, *C*).[1] For some observers—for *C* in particular—these figures formed a continuous, ever-changing series, lasting until the real form definitely appeared. For others, they came only occasionally, and often in isolated examples. Not only are the manner in which they appear, and the number of observations during which they remain unchanged, matters of great individual difference, but their number, form and variety are also most characteristic. Some 6 or 7 types of figures can be chosen for each observer, of which all the other forms seen are but modifications. The types are quite different for the different observers.

A distinction was made by the observers between these forms and those which were undoubted modifications of the real

[1] The lower part of Plate I shows the series of forms seen with a given figure. The letters indicate the observers; the numbers below them are inserted simply for convenience of reference. The figures to the right represent the form given, and those still further to the right, extending across the page, are the corresponding series of figures seen. The small *t* indicates that first the one and then the other of the figures on either side was seen during a single observation. The small numbers give the number of times that nothing was seen; the small numbers placed above the figures give the number of times that these were successively seen. *W* 1 and *C* are fragments; the number of figures in the other series has been slightly changed, but the series are none the less typical.

figure. They were spoken of as mere 'impressions.' The line of difference was, however, not sharply drawn; and its drawing did not seem to be conditioned upon any necessary or intrinsic difference of perception. (1) At times, the 'impression' shows decided marks of being influenced by the real figure, while bearing no determinate resemblance to it. It often happened, *e. g.*, in the cases of *L* and *W* (Pl. I, *W* 2, 3), that the impression gradually changed, so as to become the real figure. (2) The impression occurred less often, and was less definite, when a plain gray card was substituted for the stimulus card. When we add to these facts (3) the extreme likelihood, rooted in the nature of the method, that the observer should utilize slight retinal stimulations for the building up of the final geometrical forms, we seem to be forced to the conclusion that the 'impression' and the more objective 'perception' are identical in kind. The apparently contradictory fact, that the impression often changed entirely from one observation to another, is still not incompatible with the theory of chance local excitability of the retina. Experiments are now in progress, whose object is by variation of method to test this hypothesis, and to account (if possible) for the form preferences of the individual observers.

## § 3. The Development of the Form.

The apparatus was so disposed that, for the first few trials, the observer saw no form, or at least had no more than an 'impression.' Presently, portions of the figure began to appear. The form might begin as a mere 'suggession' of something, and then flash suddenly out; or it might develop regularly and gradually. What portion was seen first seems to have been a matter of accident, except that a long line was apt to be perceived earlier than a shorter one. The figure was at first extremely faint, in some cases coming and going so often that 4 or 5 partially different figures were seen during a single observation (Pl. I, *M*, *M* 1, *W* 3, 4). Occasionally, the whole figure would seem to move, or the lines to dance and flicker as their brightness fluctuated.[1] Gradually, in such cases, the figure would become clearer; a definitely marked outline replaced the narrow streaks of light, shading off into the background, which had at first held the attention. The illusions of § 2 were to be noticed from the moment that a form appeared at all. They might last to the very end of the experiment, though more often they disappeared as the figure became more definite. Now and again, an observer reported that he 'felt' an irregularity in the figure, before he was clearly aware of the

[1] G. M. Whipple: *Amer. Jour. of Psych.*, IX, 570.

nature of the irregularity (Pl. I, *W* 1). The simpler figures, and the more regular of the complex figures (those composed of parallel lines), were the more directly and the more correctly perceived; those figures produced the most illusions whose lines offered the most varied foundation for them.

## Summary.

(1) In looking at forms liminally different from their background, we are likely to continue lines and to complete figures, under the guidance of the two principles of symmetry and similarity. We are also likely to round angles, and to neglect certain lines altogether.

(2) On the more subjective side, we have but an indefinite idea of the number, form, and position of the component lines. Our perception is, again, guided by the principles of symmetry and similarity.

(3) Each observer has certain habits of illusion, or certain typical modes of associative completion, which persist with modification throughout his records.

# ON THE CORRELATION OF MENTAL AND MOTOR ABILITY IN SCHOOL CHILDREN.

By WILLIAM CHANDLER BAGLEY, M. S., PH. D.,
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## § 1. INTRODUCTION.

In the fall of 1897 the writer began, under the direction of Professor Jastrow, of the University of Wisconsin, a study of the correlation of mental efficiency and motor ability in school children. The studies of Hancock[1] and Gilbert[2] in Worcester and New Haven, the investigations of Peckham[3] in Milwaukee, and the excellent work of Porter[4] in St. Louis, had paved the way for a study of this kind. The radical reforms in educational method, all directed upon a more complete recognition of the motor element in school work, seemed to demand such an investigation. The old proverb, "A sound mind in a sound body," had received an infusion of new life, and the principle which it adequately expressed found an extreme form of application in the elaborate organization of college athletics. At the same time, upon the intellectual side, the rise of a certain 'muscular' school of psychophysics, and still more, the prominence of a strenuous 'motor' cult in art and fiction, had done much toward giving an unusual importance to the motor aspects of education. All this was augmented from a strictly technical standpoint by the work of Porter who, after correlating the weight of St. Louis school children with their class-room records, found a marked tendency toward a direct relation between weight and mental 'precocity.' Weight, he then argued, easily stands for motor ability; hence the child increases in mental efficiency directly as he increases in motor ability.

The investigations upon which the following study is based

[1] Hancock, J. A.: A Preliminary Study of Motor Ability, *Pedagogical Seminary*, Vol. III, pp. 9-29. (October, 1894.)

[2] Gilbert, J. A.: Researches on the Mental and Physical Development of School Children, *Studies from the Yale Psy. Lab.*, Vol. II, pp. 40-100. (November, 1894.)

[3] Peckham, G. W.: The Growth of Children, *6th Annual Report Wisconsin State Board of Health*, Vol. VI, pp. 28-73. Madison, 1882.

[4] Porter, W. T.: The Physical Basis of Precocity and Dullness, *Transactions Academy of Science*, St. Louis. Vol. VI, pp. 161-181. (March, 1893.)

are far too limited in number to admit of wide generalization. The results, however, differ so radically from those of Porter that it is thought advisable to publish them, not with the belief that they will be accepted as conclusive in any respect, but with the hope that they may throw at least a glimmer of light upon some disputed points, as well as offer some suggestions which may be of value to future investigators.

## § 2. Data.

The data of the study are drawn from four largely independent sources, two contributing the motor and two the mental data; of each of these, one source was experimental, the other non-experimental.

*A.* The Experimental Sources.

(*a*) *Motor.* The motor data were obtained experimentally by means of tests designed to determine motor ability in five respects: (1) strength, (2) rapidity of voluntary movement, (3) accuracy of voluntary movement, (4) control of voluntary movement or "steadiness" of motor co-ordination, (5) amount and character of involuntary movement. The apparatus used in the determination of these points will be described in detail later. The tests were designed to include the most important factors in motor ability, excellence in motor ability being measured by the strength, accuracy, rapidity, and steadiness of voluntary movement, accompanied by a minimum of involuntary movement.

(*b*) *Mental.* The experimental sources of the data on mental efficiency consisted of various types of reaction times as representing quantitatively the mental ability of the subject, mental excellence being represented by the alertness of the mind in reacting appropriately to given stimuli. These tests, which form an entirely independent study, were conducted by Miss Agnes Chapman, of the University of Wisconsin, as the basis for a baccalaureate thesis.

*B.* The Non-experimental Sources.

(*a*) *Motor.* The teachers in charge of the various pupils tested gave an estimate of the motor ability of the pupils as they had observed it in the process of the school work. The pupils were classed as "very clever," "clever," "medium," "awkward," and "very awkward." These terms were later translated into numerical symbols for convenience of manipulation.

(*b*) *Mental.* The mental data of a non-experimental type were derived in two ways. (1) From class-standings, as recorded in the school registers: the purely motor studies—writing and drawing—were eliminated, and the remaining standings aver-

aged upon a scale of 100 as numerical criteria of mental ability. (2) In order to eliminate the error that might arise from differences in standards used in marking by the several teachers, the estimates of the latter were again recorded,—this time the estimate of mental ability (independent of class records) being taken.

*C.* Auxiliary Data.

(*a*) *Personal.* The name, age, and grade of each pupil tested were recorded and spread upon the records of the tests together with the experimental data.

(*b*) *Anthropometric.* As material for possible correlations the following facts concerning each subject were recorded: weight in kg., height in mm., breadth of the head and length of the head,—taken at the usual points,—both in mm.

## § 3. Apparatus and Method.

Professor Jastrow's card-sorting apparatus was used in obtaining the reaction-times of the pupils. It will be found fully described, both as to structure and use, in the report of the American Psychological Association for 1897.

Following are brief descriptions of the various pieces of apparatus used in the motor tests, and of the methods employed:[1]

(*a*) The Test for Strength.

(1) *Apparatus.* For this test a dynamometer of a peculiar type was constructed. To the side of a fixed rectangular wooden frame, standing about 55 cm. from the floor, a spring balance was attached. An upward pull on the lower of a pair of grip-handles was transmitted by a lever movement to the spring of the balance. This balance was fitted with the usual recording scale; but in order to render the readings more exact the scale was discarded in practice, and its pointer used to move an arm 10 cm. long, so fixed upon a pivot that the free end described an arc of sufficient amplitude to admit of small graduations. The scale was then determined empirically, and graduated in kg.

(2) *Method.* The grip-handles were adjusted by thumb-screws to the desired distance (which, of course, varied with the size of the subject's hand); the height of the standard was not adjustable, but two small movable platforms rendered the apparatus available for subjects of all heights. Each subject was given three trials, being directed each time to grip the handles as tightly as possible with the right hand. The readings were recorded and averaged.

(*b*) The Test for Rapidity of Movement.

(1) *Apparatus.* "Trilling" a Morse key with the right forefinger was the test used to determine rapidity. The key was connected (*a*) with a dry battery of two cells, and (*b*) with an automatic recorder. The entire apparatus was securely fastened to a board (37.5 cm. square) which was placed on a table of the size used in the kindergarten.

(2) *Method.* The subject was seated before the table and directed to "trill" the key as rapidly as possible. As soon as the subject was able to manipulate the key readily, the operator switched in the current which started the recorder. The current was kept on for 10 seconds,

[1] The interested reader may be referred to the discussion of apparatus in the course of a study on a topic related to the present, by W. L. Bryan, *Amer. Jour. of Psych.*, V, pp. 125-204.

at the end of which time the operator opened the switch, the subject continuing to operate the key for a few seconds after the current had been shut off. Three successive records were taken in this way, and the results averaged.

(*c*) The Test for "Steadiness" of Motor Co-ordination.

(1) *Apparatus.* For this test a complicated scroll-plate was devised. A thin sheet of tin-foil was carefully smoothed out and waxed upon a piece of plate glass. Upon this foil a scroll diagram was first traced out and then cut, leaving a slit 1 mm. wide. This slit was continuous through all the complications of the scroll, but for convenience its course was divided into four sections, each representing a peculiar type of movement. The positive pole of the battery was connected with the edge of the foil, the negative pole with a tracing needle made by inserting a sewing needle into the end of a pencil holder and connecting it with the battery by means of a fine wire.

(2) *Method.* The scroll-plate was fastened upon the board used in the rapidity test, the subject was seated before it, given the pen, and directed to trace the pattern, being cautioned that he was to keep to the middle of the slit, every failure to keep away from the foil being recorded by a tap of an electric bell which was thrown into the circuit. The taps of the bell were recorded for each section of the scroll, as well as the time for each section, the total number of taps, and the total time.

(*d*) The Test for Accuracy and Constancy of Voluntary Movements.

(1) *Apparatus.* This test was made by a recording target (30 x 30 cm.) mounted upon a table (like the one used in the last two tests), the target being inclined backward to make an angle of 45° with the table-top. The target consisted of a wooden frame with a solid back, the frame being fastened to the back by means of hinges. Into this frame a sheet of paper was inserted having marked upon its center a black bull's eye 10 mm. in diameter. Behind this were a sheet of carbon paper and a sheet of record paper upon which the impressions were preserved.

(2) *Method.* The subject was placed two m. away, facing the target, and was prevented from approaching nearer by a movable upright. He was given ten marbles and directed to toss them, one at a time,—attempting to strike the bull's eye at each trial. After the ten marbles had been tossed the sheet was removed (after first receiving the impression of the bull's eye for reference in later measurements). This process was twice repeated, making three records in all.

These records were treated in two ways. In the first place the distance of each impression from the center was measured in mm. for the determination of the constant error of each sheet. The constant errors of the three sheets belonging to each subject were then averaged, and the result recorded as an index of motor accuracy. Then the average error was computed (per cent.) for each sheet, the three percentages of each subject being again averaged as an index of constancy of movement.

(*e*) The Test for Amplitude of Involuntary Movement.

(1) *Apparatus.* The apparatus used for determining the amplitude of involuntary movement was the automatograph designed by Professor Jastrow and known by his name. A full description will be found in the *American Journal of Psychology*, Vol. IV, pp. 398-407.

(2) *Method.* The automatograph was placed upon the table, the subject being directed to stand before it in such a position that the median plane of his body made an angle of 45° with the edge of the

automatograph opposite the recording pencil. The attention of the subject was then concentrated upon a metronome placed upon a table two m. in front of him. This metronome was set at 120 beats per minute, and the subject was directed to count the beats up to 120, raising his right hand and resting it, tips of the fingers down, upon the upper plate of the automatograph, when he began to count. At the same time the operator dropped the pencil upon the recording plate, leaving it there until the subject had completed the counting. Three records of this kind were taken. The total amplitude of movement was measured upon each record by means of a sliding compass, the average of the three record-amplitudes thus obtained being taken as an inverse index of the subject's excellence in the test. While every precaution was taken to make these measurements as exact as possible, they must still be interpreted rather as approximations than as exact determinations.

## § 4. Results.

The tests were begun December 13, 1897, at the Fifth Ward School, in Madison, and continued daily (during the school sessions) until the first of May, 1898. Each test occupied from twenty-five to forty-five minutes, the average being about thirty-five minutes. In all one hundred and sixty tests were made for motor ability, and one hundred and seventy-five for mental ability (reaction-times). Of these two series of tests one hundred and ten were upon the same pupils. For both tests additional data were collected from the school records and from the teachers' estimates as described above, but only for those who underwent the motor tests were anthropometric data tabulated.

*Method of Treating the Data.* The entire 'motor,' 'personal' and 'anthropometric' data were spread upon four large sheets of ruled paper. In this way the material was arranged in such a manner as to render it easy of manipulation. To the data thus arranged were added from time to time the class standings of the subjects as obtained from the teachers' registers and the teachers' estimates of mental and motor ability as translated into numerical terms, together with the "indices" (to be described later) of the mental alertness and motor ability of each subject as gained from the tests upon reaction-times, and from the motor tests.

The data as thus arranged were first examined with a view to determine general relations. The various columns were averaged, and from these averages curves of distribution were plotted.

Aside from this general treatment special correlations were made in the following manner.

The quantitative results of each important test were divided into five classes of thirty-two subjects each, the thirty-two having the highest records in each case being placed in the first class (designated Class AA), the thirty-two having the lowest being grouped in the last class (Class XX), while the remain-

ing ninety-six were similarly grouped into three intermediate classes (Classes A, M and X) in the order of their excellence. The data as arranged in this way for each test were spread upon separate sheets (one sheet for each class), and upon these same sheets were also placed the remaining data—mental, motor, personal, or anthropometric—which we wished to correlate with the given test, each line of figures across the page representing the results given by the same subject in the various tests. The vertical columns were then averaged, and a single correlation was completed.

An example of the process may make it clearer. We will suppose the results of the dynamometer test to have been arranged in five groups as just described. It is desired to correlate with this dynamometer test the standing obtained from the school registers. Along with the transfer of the dynamometer standings from the general sheet to the correlation sheet, the class standings are also transferred, the dynamometer record of each pupil being placed upon its appropriate sheet and the class standing of the subject represented being placed in an adjacent column. When the vertical columns are averaged, the general average dynamometer standing of the thirty-two subjects who were the best in the test is shown, and beside it is the average class standing of these thirty-two best dynamometer subjects. By this means, after averaging the remaining sheets in the same way, we arrive at what we may term a single or simple correlation, that is, a correlation of five groups, of a more or less uniformly varying scale of excellence in the given test, with the average class standings of these same groups. From this a curve may be plotted, the five averages of the correlating test being the five points of the curve as measured from one co-ordinate in terms of the five averages of the data with which the given test is correlated.

This method may be complicated by the following process. After making a simple correlation as above, arrange the data which have been correlated with the given test in five similar groups of thirty-two subjects each, arranged in the order of excellence as represented by these data.

To continue the above instance. If, after correlating the dynamometer test with the class standings singly as was described, it is wished to make a double correlation, the class standings may be arranged in five classes according to their degrees of excellence. Beside each subject's class standing, his dynamometer test may be recorded. These sheets, averaged as before, will give the average dynamometer results for each group of subjects arranged in order of class standings. If a curve is plotted from this latter correlation upon the same co-ordinates as the curve of the former correlation, and if care is taken to have the order of excellence in each case proceed from left to right upon the abscissa, and from below upward upon the ordinate, the following conditions may be observed:

(1) If the two curves have a general N. E. (northeasterly) direction (*i. e.*, from the meeting point of the co-ordinates to the upper right hand corner of the cross-section sheet), the correlation is direct; *i. e.*, the order of excellence in the one case bears a direct relation to the order of excellence in the other case.

(2) If the curves have a general S. E. (southeasterly) direction, the

correlation is antagonistic, *i. e.*, the order of excellence in the one case bears an inverse relation to the order of excellence in the other case.

(3) If the curves cross each other at right angles, the correlation is indifferent, the degree of indifference depending upon the degree to which the angle approaches 90°.

It may be well to add that these conclusions can be reached less satisfactorily from the single correlations, the general direction of the single curve giving some index as to the character of the correlation,—always providing that the order of excellence is as described above.

*The Motor and Mental Indices.* The terms "motor index" and "mental index" appear in several places in the Tables. The motor indices were derived as follows: The results of each test were arranged in the precise order of their excellence; the highest was then given an arbitrary value, 999, the others were given values in order down to 840 (which exhausted the 160 subjects). The process was completed for five tests, viz., the tests for rapidity, accuracy and steadiness of voluntary movement, the test for strength, and the test for amplitude of involuntary movement. Then these five motor standings of each subject were averaged, and an arbitrary symbol was obtained which represented the motor ability of each subject. This was called the motor index. Proceeding in the same general way we obtained a mental index from the mental tests and from the class-records. This mental index was taken to represent mental alertness, the handling time involved in the reaction experiments having been determined and eliminated.

*The Teachers' Estimates.* These were translated into numerical terms according to the following scheme. Motor ability: very clever, 5; clever, 4; medium, 3; awkward, 2; very awkward, 1. Mental ability: very bright, 5; bright, 4; medium, 3; dull, 2; very dull, 1.

## § 5. Interpretations and Conclusions.

### (*a*) Comparisons of the various Motor Tests with the Class Standings. Double correlations.

In the following Tables the data mentioned first form the criteria for the division into the classes,—AA, A, M, X and XX. That is, in the first part of the following Table the class AA represents the thirty-two subjects who stood highest in the test for strength; the average of the class standings of these thirty-two is given directly below the figures which indicate the number of kg. which they registered on the dynamometer. In the second part of the Table the class AA represents the thirty-two who had the highest class standings. The average dynamometer 'pull' of *these* thirty-two is placed directly below their average class standing.

TABLE I.

*Correlation of the Test for Strength and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Dynamometer, | 33.0 | 25.9 | 22.0 | 18.0 | 13.1 |
| Standings, | 75.4 | 80.0 | 83.97 | 85.71 | 82.92 |
| Standings, | 92.7 | 85.7 | 83.0 | 74.0 | 67.9 |
| Dynamometer, | 21.3 | 19.7 | 22.3 | 24.7 | 25.8 |

The above Table shows a decidedly inverse relation between mental ability, as indicated by class standings, and motor ability, as indicated by the dynamometer records.

TABLE II.

*Correlation of the Test for Rapidity of Voluntary Movement and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| 'Trilling,' | 65.51 | 56.96 | 53.89 | 48.42 | 40.33 |
| Standings, | 80.31 | 82.18 | 85.90 | 79.39 | 79.30 |
| Standings | 92.7 | 87.5 | 83.0 | 74.0 | 67.9 |
| 'Trilling,' | 52.3 | 50.7 | 53.4 | 51.6 | 53.6 |

This Table, in both its parts, is not conclusive. If the curve were plotted it would be 'indifferent.'

TABLE III.

*Correlation of the Test for Steadiness of Voluntary Movement and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| 'Tracing' Errors, | 68.6 | 115.28 | 145.5 | 176.7 | 229.2 |
| Standings, | 78.1 | 81.44 | 77.1 | 82.45 | 90.88 |
| Standings, | 82.7 | 87.5 | 83.0 | 74.0 | 67.9 |
| 'Tracing' Errors, | 164.8 | 145.0 | 133.1 | 135.1 | 136.3 |

A curve plotted from Table III would show a decidedly inverse relation between mental ability, as indicated by class standings, and motor ability, as indicated by the tracing test.

TABLE IV.

*Correlation of the Test for Accuracy of Voluntary Movement and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| 'Target' Test, | 74.2 | 99.3 | 112.2 | 127.7 | 149.0 |
| Class Standings, | 78.2 | 78.5 | 84.3 | 81.8 | 83.9 |
| Standings, | 92.7 | 87.5 | 83.0 | 74.0 | 67.9 |
| 'Target' Test, | 113.1 | 116.2 | 108.4 | 95.9 | 102.5 |

This Table, like the preceding, shows a predominantly inverse relation between motor ability and mental efficiency. The curve which might be plotted from it would, however, be less uniform than that of Table III.

TABLE V.

*Correlation of the Test for Amplitude of Involuntary Movement and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Automatograph, | 46.3 | 69.9 | 86.9 | 98.9 | 137.9 |
| Standings, | 81.8 | 81.9 | 81.4 | 82.5 | 79.0 |
| Standings, | 92.7 | 87.5 | 83.0 | 74.0 | 67.9 |
| Automatograph, | 86.0 | 83.2 | 93.4 | 83.6 | 82.8 |

The curve from Table V would probably be classed as 'indifferent,' although it would show a very slight tendency toward the 'direct' relationship.

TABLE VI.

*Correlation of the Test for Constancy of Voluntary Movement and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Constancy Index, | 29.12 | 34.3 | 39.7 | 42.8 | 51.7 |
| Standings, | 81.51 | 85.03 | 76.73 | 83.50 | 81.6 |
| Standings, | 92.7 | 87.5 | 83.0 | 74.0 | 67.9 |
| Constancy Index, | 40.5 | 40.3 | 39.5 | 41.8 | 38.9 |

The curve for Table VI would be rather less 'indifferent' than the curve for Table V, and the tendency would be toward an 'inverse' relation.

TABLE VII.

*Correlation of the General Motor Index and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Motor Index, | 961.8 | 938.3 | 924.3 | 909.0 | 881.9 |
| Standings, | 77.8 | 80.0 | 83.6 | 83.8 | 84.7 |
| Standings, | 92.7 | 87.5 | 83.0 | 74.0 | 67.9 |
| Motor Index, | 917.2 | 907.1 | 922.8 | 931.0 | 931.05 |

In this Table the individual discrepancies of the preceding Tables are eliminated, and the curve would show a very marked inverse relation between motor ability, as represented by the various tests, and mental efficiency, as represented by the class standings.

(*b*) Comparisons of the Teachers' Estimates of Mental Ability with Class Standings and with the Motor Index.

TABLE VIII.

*Correlation of Teachers' Estimates of Mental Ability and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Estimates, | 4.62 | 4.0 | 3.16 | 2.87 | 1.59 |
| Standings, | 90.5 | 86.28 | 77.0 | 82.43 | 71.9 |

This single correlation indicates that there is no appreciable discrepancy between the teachers' estimates of mental ability

and the values taken from the class records as representing mental ability.

TABLE IX.

*Correlation of the Teachers' Estimates of Mental Ability and the Motor Index.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Motor Index, | 961.8 | 938.3 | 924.3 | 909.0 | 881.9 |
| Estimates, | 3.32 | 3.15 | 3.46 | 3.36 | 3.34 |
| Estimates, | 4.62 | 4.0 | 3.16 | 2.87 | 1.59 |
| Motor Index, | 908.0 | 924.4 | 901.6 | 913.3 | 926.8 |

While this Table lacks the uniformity of Table VII, the inverse relation is more than indicated.

(*c*) Comparison of Reaction Times with Class Standings and with the Motor Index.

TABLE X.

*Correlation of Reaction Times and Class Standings.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Reaction Times, | 13.5 | 15.7 | 17.1 | 19.0 | 22.7 |
| Standings, | 78.8 | 82.2 | 80.6 | 82.3 | 77.3 |

This Table shows a discrepancy between the indices of mental ability as represented by class standings and by reaction times. The relation, however, is not inverse, but rather indifferent, the children whose reaction times are nearest the norm having the best class standings, while those who are particularly alert and those who are particularly slow in reaction are alike deficient in mental efficiency as represented by the class standings.

TABLE XI.

*Correlation of Reaction Times and Motor Index.*

| CLASS. | AA. | A. | M. | X. | XX. |
|---|---|---|---|---|---|
| Reaction Times, | 13.5 | 15.7 | 17.1 | 19.0 | 22.7 |
| Motor Index, | 928.6 | 930.9 | 935.2 | 928.6 | 935.2 |

Table XI, like Table X, is inconclusive, but shows a tendency toward an inverse relation between mental ability, as represented by reaction times, and motor ability, as represented by the motor index.

(*d*) The Elimination of the Age Factor.

In the above determinations the tendency toward an inverse relation between mental and motor ability is quite decided, even when mental ability is represented by two values as indifferent as regards each other as are the reaction times and the class standings. The factor of age, however, has not been considered, and it might very well be possible that this variable factor would compensate the differences which we have found. The

results which were obtained demonstrated conclusively that general motor ability increased with growth, but the relation between growth and mental ability was not so clearly shown. There is no alternative, therefore, but to eliminate in one way or another the factor of age. To do this the following course was pursued. The subjects were divided into five groups of thirty-two each upon the basis of age, the thirty-two oldest subjects being placed in Class AA, the thirty-two youngest in Class XX, and the remainder divided between Classes A, M and X in the order of age. Each of these classes was then treated exactly as the entire one hundred and sixty subjects were treated in the first instance, except that each was divided into four classes of eight subjects each, instead of into five classes of forty subjects each. The principle of division varied according to the information which was desired. Each Class (AA, A, M, X, and XX) was, for example, divided into groups of eight with regard to the motor index, or with regard to weight, or with regard to mental ability. In the last named case a slight departure was made from the former treatment. A 'mental index' was established for each subject in exactly the same way that the motor index had been established, except that only the class standings and the reaction times were used in the determination. By the aid of these mental and motor indices the comparison of mental ability and motor ability can be made with much more nicety than was possible under the former procedure.

TABLE XII.

*Comparison of Mental and Motor Ability with Age Factor eliminated.*

| | Range of Age. | Av'age Age. | Class AA. (Best Average Motor Index.) | | Class A. (2nd best Av. Motor Index.) | | Class X. (3rd best Av. Motor Index.) | | Class XX. (Lowest Av. Motor Index.) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | Motor Index. | Mental Index. | Motor Index. | Mental Index. | Motor Index. | Mental Index. | Motor Index. | Mental Index. |
| AA | 14–17 | 14.79 | 962.1 | 894.6 | 941.2 | 894.7 | 930.5 | 927.3 | 908.0 | 929.3 |
| A | 13–14 | 13.7 | 967.9 | 903.6 | 947.8 | 925.8 | 935.2 | 919.8 | 899.6 | 930.7 |
| M | 12–13 | 12.53 | 943.3 | 939.5 | 921.0 | 927.6 | 912.4 | 921.2 | 887.6 | 901.6 |
| X | 12–12 | 12.0 | 944.8 | 952.2 | 931.1 | 950.1 | 912.8 | 942.5 | 881.4 | 938.5 |
| XX | 8–11 | 10.6 | 926.5 | 944.5 | 902.2 | 948.3 | 889.9 | 958.0 | 874.5 | 934.2 |
| | Averages. | | 928.9 | 906.8 | 928.6 | 909.3 | 916.1 | 933.7 | 890.2 | 926.8 |

1st half, average motor index, . . . . . 928.7
" " " mental " . . . . . 908.0
2d half, average motor index, . . . . . 903.1
" " " mental " . . . . . 930.2

The above Table shows very plainly the general tendency toward an inverse relation between mental and motor ability.

But it also shows that the law—if it be a law—is subject to numerous individual variations; and the discrepancies, especially in Class X, indicate that at certain periods of growth the relation between the two factors may be quite the reverse of the relation in general.

TABLE XIII.

*Comparison of Weight, Class Standings and Motor Ability with the Age Factor eliminated.*

(Range of age and average age same for each class as in Table XII.)

| | | Class AA { Highest Av. Weight. | | | Class A { 2d Highest Av. Weight. | | | Class X { 3d Highest Av. Weight. | | | Class XX { Lowest Av. Weight. | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Weig't. | Motor Index. | Class Standings. | Weig't. | Motor Index. | Class Standings. | Weig't. | Motor Index. | Class Standings. | Weig't. | Motor Index. | Class Standings. |
| Oldest | AA | 131.7 | 947.6 | 73.2 | 113.1 | 926.3 | 78.3 | 100.4 | 935.9 | 70.8 | 86.0 | 939.2 | 74.5 |
| 2d Oldest | A | 105.1 | 930.4 | 74.8 | 92.6 | 958.2 | 77.7 | 86.9 | 929.7 | 74.2 | 77.2 | 929.3 | 78.5 |
| 3d Oldest | M | 90.5 | 915.6 | 80.0 | 81.5 | 918.9 | 83.0 | 75.0 | 919.5 | 84.2 | 65.4 | 911.2 | 85.1 |
| 4th Oldest | X | 101.2 | 922.6 | 85.2 | 88.2 | 912.2 | 85.6 | 78.2 | 920.3 | 90.5 | 69.8 | 909.4 | 83.8 |
| Youngest | XX | 89.2 | 905.2 | 85.2 | 73.2 | 896.7 | 88.7 | 66.8 | 894.2 | 84.7 | 57.7 | 897.6 | 86.2 |
| Averages | | 103.7 | 924.2 | 79.6 | 89.7 | 922.4 | 82.6 | 81.4 | 919.9 | 80.8 | 71.2 | 917.3 | 81.6 |

1st half, average weight, . . . . . 96.7
" " " motor index, . . . . . 923.3
" " " class standings, . . . . . 81.1

2d half, average weight, . . . . . 76.3
" " " motor index, . . . . . 918.6
" " " class standings, . . . . . 81.3

This Table does not indicate such a direct relation between weight and mental ability as Porter found in his investigations upon St. Louis school children. On the other hand,

the relation between weight and motor ability, with the age factor eliminated, while slightly direct, is mainly indifferent.

(*e*) Other Correlations.

The correlations which have been given above did not exhaust the data collected. Each set of determinations—personal, anthropometric, motor and mental—was correlated with every other set of determinations, with the hope of discovering some uniform relation between the various factors. To present the complete Tables exhibiting these correlations would, of course, be quite out of the question within the limits of the present paper. Some of the more important conclusions are, however, indicated in the following summary.

## Summary.

1. Under the conditions of the investigation, and with the children that were tested, there is a general inverse relation between motor and mental ability; those who are the 'brighter' pupils and those who have the quicker reaction times being, as a rule, deficient in motor ability, while those who are best developed physically, who are the strongest, who have developed motor 'control' to the greatest extent, are generally deficient in mental ability. This rule, however, was found, with the children tested, to have numerous individual exceptions, and a varying validity at different periods of development.

2. There seems to be little direct relation between mental ability as represented by reaction times, and mental ability as represented by class standings, except that excellence in either of these directions is apt to be accompanied by a deficiency in motor ability.

3. There is a gradual increase of motor ability with age. The increase in mental ability is not so well marked.

4. In general, the boys slightly surpass the girls in motor ability, while the reverse obtains in mental ability.

5. Regarding cranial capacity as indicated by the head girths, we notice a significant trend toward an inverse relation between mental ability and head girth.

# EXPERIMENTAL STUDY OF THE MENTAL PROCESSES OF THE RAT. II.

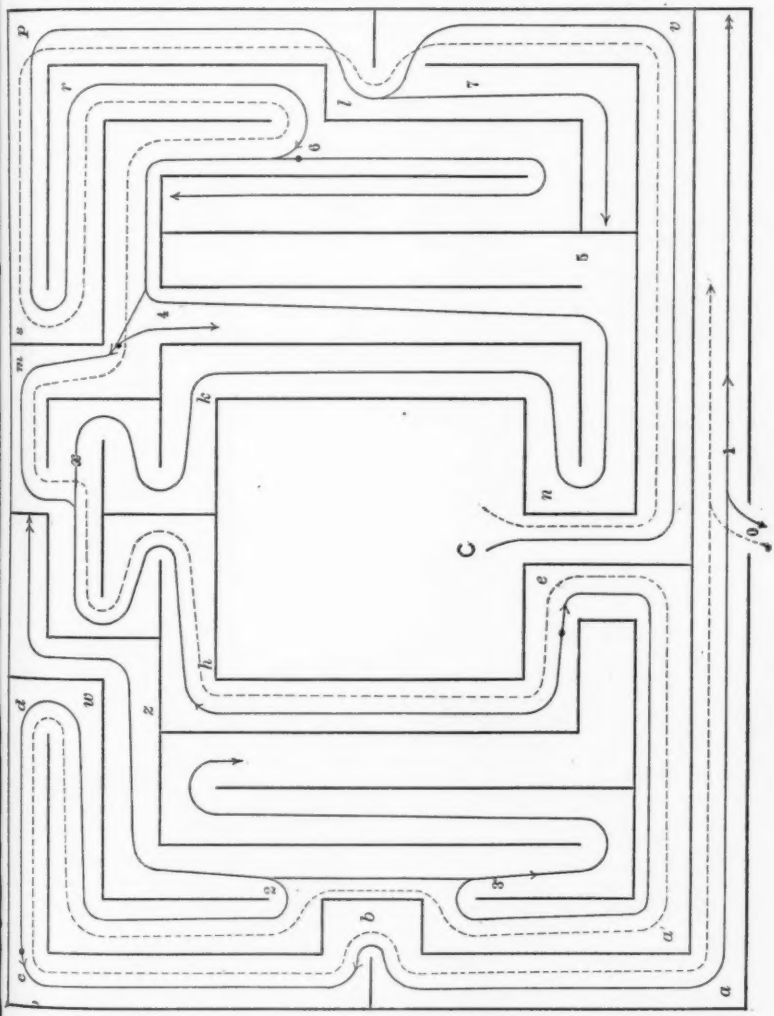
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The present paper, which supplements, in a measure, two former papers, (*Am. Jour. Psy.* Vol. XI, Nos. 1, 2), presents in detail the results of some further experimental studies upon the mental life of the rat. Primarily a study in method, an attempt to observe this animal under approximately experimental conditions, the methods and devices have worked well enough to warrant considerable confidence in the results, and the inferences therefrom. The paper describes the apparatus used and the conditions of experiment, gives a detailed account of a typical series of experiments, compares the intelligence of wild rats and the tame white rats (same variety) upon the basis of these experiments, discusses briefly the general form or character of animal intelligence, and makes some suggestions in regard to the mental facts involved in solving the problems set in the tests. It is not in any sense a systematic rat-psychology.

## APPARATUS AND CONDITIONS OF EXPERIMENTS.

The aim in these experiments, as indicated above, was to make observations upon the free expression of the animal's mental processes, under as definitely controlled conditions as possible; and, at the same time, to minimize the inhibitive influence of restraint, confinement and unfamiliar or unnatural circumstances. Fear, which in lack of a better term, may be used to include the three influences just noted, and too great difficulties are the things most rigorously to be guarded against. On the positive side, the experiments must conform to the psycho-biological character of an animal if sane results are to be obtained. This is not the same thing as guarding against "too great difficulties." The difficulty of two tasks, judged by their complexity and the quality of intelligence necessary for their performance, may be identical; yet the problem involved in the one may be so different from that in the other, so remote from the animal's racial experience and life habits as to be absolutely outside his capabilities. A human being has to know how the elements of an action feel—or better, has to know the *feeling* of the elements—in order to perform the action. The

importance of these characteristic differences is obvious enough where structural differences provide the basis: no one would



expect a buffalo and a rabbit to do the same things in the same way. No less important, though less easy to define and designate, are the specifically psychic traits of an animal which constitute its character, and which depend in the main upon its biological conditions. The cat and the rat are not antithetical in structure, nor do they differ very widely in degree and kind of intelligence; but their life habits, both on the emotional and the intellectual side, present an effective contrast. The cat, primarily a hunter, is bold, independent, and aggressively open; the rat, on the contrary, primarily the hunted, and only secondarily a hunter, is timid and furtive. His timidity is comparable to an intellectual obsession. His boldness when displayed is impudent and half apologetic, never self-contained and unconscious like that of the cat. I daily see this matter exemplified in the case of a pet rat and a young cat. Though the rat has compelled the cat to respect his rights, the characteristic difference of mental attitude is not greatly changed.[1]

Conforming with such considerations, appeal was made to the rat's propensity for winding passages. A recent magazine article upon the Kangaroo Rat, by Mr. Ernest Seton Thompson[2] illustrates well the radical character of this rodent trait. Mr. Thompson gives a diagram of the Kangaroo Rat's home-burrow, the outline of which bears a striking resemblance to that of the apparatus used in these experiments. It suggests that the experiments were couched in a familiar language. Not only do they conform to the sensori-motor experience of the animals, but they also fall in with their constructive instinct relative to home building.

The Hampton Court Maze[3] served as model for the apparatus. The diagram given in the Encyclopedia Britannica was corrected to a rectangular form, as being easier of construction. The character of the problem was not affected. Three mazes were made. The first was as follows. The dimensions were 6 by 8 feet. The bottom was of wood, the boards being fastened together so as to make a portable whole of the apparatus. All the rest: top, sides, and partitions between galleries were of wire netting, ¼ in. mesh. The height of the sides was 4 inches; the width of galleries, the same. In the center was a large open space. The accompanying diagram gives the ground plan of the maze. The entrance is marked by the figure *O*. Figures *1* to *7* indicate seven blind alleys—seven

[1] The character of the white rat is modified somewhat by its immemorial life of captivity, but less than might be supposed. All the primitive traits emerge under the appropriate conditions.

[2] Scribner's Magazine, April, 1900.

[3] *Cf.* Ency. Brit. Art. "Labyrinth."

possibilities of error. It will be observed that *4* does not lead necessarily into the *cul-de-sac 5*, but does inevitably furnish a chance for error. The letter *x* marks a dividing of the ways either of which, however, may be followed without completely losing the trail. Certain other points are indicated by letters *a*, *b*, *c*, etc., for convenience in description. C stands for center. A glance at the maze will be sufficient to convince one of the difficulty of the problem. This maze is designated as Maze I. The two other mazes were identical with this, except that they were made throughout of wire netting, bottom as well as top and sides. The apparatus could thus be reversed and a mirror reproduction of the original form could be obtained. In use they were fastened temporarily to wooden floors. These mazes will be spoken of as II and III. In all cases the mazes were placed upon tables 2½ feet above the floor. The floor of the maze was covered with sawdust. This was renewed whenever a new rat was introduced.

To obviate the affective disturbance which would have resulted from change of conditions just prior to experiment, as little difference as possible between the conditions of experiment and of ordinary experience was allowed. The event frequently justified the precaution. The rats were kept in an ordinary observation cage,[1] a well ventilated and commodious apartment, standing flush with the maze at *O*. Back of the cage, upon the same table, was a screen completely concealing the observer during experimentation, but permitting him to look down upon the maze and mark every movement of the rats. When ready for experimenting, the sliding glass door which closed the passage into the maze was lifted by means of a pulley connection running over the screen. This door always opened with slight noise and friction. Food was placed in the central enclosure. To obtain this, the rats had to find their way through the tangled maze. They were kept hungry enough so that they would set about the task vigorously. Experimenting was done in the evening to minimize the influence of distracting noises.

In spite of these precautions there was, as might be expected, some lack of uniformity in the results—so much has to be allowed to the internal conditions and to individual variation.

As a beginning, two series of experiments were made; one with wild brown rats, *mus decumanus;* the other with white rats, albinos of the same variety. The original purpose was to use the wild rats exclusively; but the difficulties were considerable, and the white rats were found to serve the purposes of experiment quite as well in every way. In all the experiments after the first series white rats were used. The most of the later

[1] *Cf. Am. Jour. Psy.*, X, 3, p. 135.

experiments were made to determine special points, and will be described in their immediate connections.

As before stated, the rats were let into the maze at the time of experiment and were left until the following morning. The entire night was theirs for investigating the maze. This method excludes the possibility of a quantitative study, and intentionally, for reasons given within.

### Description of Experiment.

In presenting the results of these experiments I give a detailed account of a typical series, following this with explanatory suggestions. The conduct of the rat is described as it was minutely recorded at the time of observation. This is supplemented in passing by comparison with other experiments and by details taken from them. This plan of presentation has the advantage of exhibiting in detail the method of animal intelligence in actual operation,—it is analogous with vital staining in histology—rather than in figuring its general form as could be done by the method of graphic representation in curves. Again, in view of the rôle one's own prepossessions and limitations are bound to play in interpretation, it will be an advantage for the reader to know the observed facts upon which the writer's interpretations are based. I readily concede that my facts may be found more valuable than my inferences. Be that as it may, all who are interested in comparative psychology will agree that careful and impartial description of the objective facts of animal intelligence is a desideratum.

I have selected for detailed description my second series of experiments. The subjects were two young male white rats, about 3½ months old. They were perfectly tame—pets from birth.[1] Nine experiments were made at intervals of two days; and a tenth after a lapse of three weeks. The conditions were the same as with the wild rats of the first series.

Before giving this detailed description I wish to present a brief summary of the observations upon the wild rats in series 1, in order to make clear the passing comparison, given within, between the wild and the tame rats. This comparison is purely a by-product of the study—it was not contemplated at all in the beginning—and it takes us a little out of the straight line of exposition; but the results have a suggestive value sufficient to warrant the delay.

The subjects of Series I were three adult wild rats—a male and two females. They had been in captivity about three months, but were in perfect physical condition. No particular

[1] Rats A and B of Group V, reported in *Amer. Jour. Psy.*, Vol. XI, No. 2, pp. 156 ff.

effort had been made to tame them except by excluding as far as possible occasion of fright. They remained so wild, however, as to necessitate the extreme precautions described above.

The first two experiments failed of definite results because of imperfect conditions. The rats were frightened by external disturbances, so that they failed to move (Exp. 1,) for more than 10 m., and did not reach C in two hours. This they accomplished during the night in each case. It was not until Exp. 3 that the conditions were made to conform perfectly to the description above. After that, observation was made and results recorded more satisfactorily. The influence of the first two trials is apparent in Exp. 3, the journey being made in 15 m., and with 8 errors.[1]

In this experiment and in all succeeding experiments of the series the male showed striking superiority to the females. In this case they did not reach C until 15 m. after the male—indeed they did not leave the cage till after he had reached C and was well on his way back to the cage. He met one of the females at *c*. Under the same stress of hunger, they exhibited throughout the series very much less boldness and initiative. This humbler character comes out again in the deference the females showed the male. Whenever he was monopolizing the food they would make furtive attemps to steal the prize, or approach with deprecating squeaks as if begging to be allowed to share. On several occasions I saw him chastise a female severely for trying to get the food away. The females, too, were less apt in familiarizing themselves with the maze. In the fifth experiment, I observed that they showed little familiarity or assurance, and seemed much more disconcerted than the male when they got into a blind alley. The male died after Exp. 7, but even after that the improvement of the females was relatively slow. Four more experiments showed slight gain. Their timidity seemed to abate but little, and prevented them from giving full attention to the problem.

Later tests with male and female white rats gave the same general result, though the difference was not so marked. In only one case out of ten or twelve pairs observed did the female show equal initiative. The significance of this is not, I think, that the intelligence of the males is higher or more refined, but, rather, that it is more effectual by reason of being less subject to affective disturbances and inhibitions. The case is analogous with a point made later in comparing the white and the gray rats: the appearance of greater intelligence really may mean

[1] By errors I mean the returns—*e. g.*, going back from *d* to *a*—as well as the positive errors of following wrong paths.

nothing more than that one animal is somewhat more energetic than another, moves more and faster, and consequently has more chances of succeeding in a trial and error series.

It is interesting to note, also, that domestication tends to reduce this disparity between the sexes. It suggests the very live question whether civilization, which is a kind of domestication, operates similarly with the human species. Popular opinion seems to lean towards the affirmative; and we find expression of this movement of the social mind in the tendency to identify the aims, conquests, and education of the sexes. On the other hand anthropologists recognize a progressive differentiation of the sex types, physiologically and biologically. Possibly a point in development has been reached where psychological development has attained a greater freedom than heretofore from physiological and biological determinants.

But to return from this digression. The male (m) showed the influence of the previous experience not only in the reduction of time and errors, but also manifestly by avoiding several errors and by hesitation at certain points. He even returned to the cage without any mistakes. In some parts of the maze the movement had some definiteness, but generally had a groping appearance. This in itself is significant, suggesting that already he passed beyond the stage of purely accidental selection, that he had already acquired some kind of recognition, however vague, of rights and wrongs. The selection of paths begins to be purposive. In the four succeeding trials, in which this rat was a subject, there was observable a constant increase in the purposiveness of the movements. This takes the form of definiteness and speed where no error was possible, and of doubt and indecision where there was choice of path. The indecision tended to fade away, but did not disappear completely in this (too) brief series.

If the matter were illustrated graphically by curves, we should have the curve of speed and definiteness beginning with a maximum of slowness and complete indefiniteness, and sweeping down gradually to relatively perfect definiteness and a minimum of time. The curve of choice, on the contrary, would begin very low, rise gradually as the recognition of critical points became more acute, and would then fall gradually as the right association became habitual.

As further illustrative of this increasing purposiveness I note from the records the following pertinent observations. In Exp. 4, "one could not fail to see that the rat was trying to select his path—there seemed to be some kind of an image in his mind that he was trying to follow. The first time he came to *4* he hesitated, then went wrong; each succeeding time he seemed to recognize this place, for he went by confidently.

Indeed after the first time around he seemed sure of his path beyond this point, and at *s* visibly accelerated his pace each time." In Exp. 5, he made fewer errors, and recoiled more quickly from an error when made. Movements were all rapid, yet cautious. Again made "the error at *4*, and again quickened his pace as he passed it the second time. This acceleration was accompanied by a flick of the tail and a general abandon that said, 'I've struck the right trail.'" The movements in Exp. 6 "were purposive. He apparently knew when he was on the right and when on the wrong road. The right (latter) half of the maze seemed better in mind than the left; before reaching *m* the movements were cautious and uncertain—several mistakes—but beyond *m*, they were rapid and secure—no mistakes." Exp. 7 gave similar results.[1] There is little reason to doubt that a few more trials would have enabled him to learn the way perfectly as did the white rats in many later experiments.

The time and error factors of the five experiments, which were accurately recorded, were as follows:[2] III—15, 8′. IV—10, 2′. V—1¾, 4′. VI—3, 4′. VII—⅝, 3′. It will be seen that the relative decrease in time is more nearly constant than that in number of errors—*e. g.*, Exp. *4*, which required 10 m., gave only 2 errors; Exp. 7, requiring only 50 seconds, gave 3 errors. Probably the small number of errors in Exp. *4* is largely accidental. There is doubtless a somewhat constant relation between knowledge of the path (indicated by relative number of errors) and the time required to traverse it. It must be remembered, however, that organic conditions vary so as to make this relation practically indemonstrable.

In this series of experiments, the writer failed to observe any thing that would indicate that the sense of smell played an important rôle in the process of solving the problem, at least so far as following a trail is concerned. In no case could I make out that the male followed his own latest track or the females followed the track of the male, which often would have led them almost directly to the goal. All later experiments confirmed this view. The evidence is given in detail in its proper place *infra*.

It should be remarked, also, that the females gave no evidence of intelligently following the male, when by doing so they might have gone directly to C. The rats passed and repassed each other, each going his or her own way. Occasionally one would be deflected from his path to follow another, but seldom for any considerable distance. This same independence was observed

[1] Reason for discontinuance was death of the rat—disease unknown.

[2] Roman numerals indicate the experiment number; the plain Arabic, the time in minutes; the accented Arabic, the number of errors.

in all later experiments, so that I think imitation even in its simplest form[1] cuts a very insignificant figure in this matter.

One other matter of interest is the general conduct of the rats in the maze. I have spoken already of the initial timidity. The manner soon became more confident, affective tension was relaxed, and curiosity and the play instinct were unloosed. In Exp. *3*, after the rat[2] had eaten and drunk a little, he seemed to become thoroughly happy, and, for the nonce, quite oblivious of the world of traps and snares. He alternately ate, drank, and ran curiously about the maze, investigating all the passages in a sprightly and eager manner. In Exp. *4*, I note "the restless curiosity they all manifest after the first pangs of hunger are appeased, carefully exploring all the passages, feeling and sniffing along the sides and tops and into all the angles and corners. Their conduct impresses one strongly as being the expression of a free curiosity—a fundamental and irreducible desire to know all this new environment. It has not at all the appearance of a further search for food, but rather a seeking for the feeling of security and at-homeness attendant upon knowledge of surroundings.

I pass on now to describe in detail Series II, with white rats.

*Exp. 1. A.* 13 m. *A* came out of cage 2 m. after the door was opened. A few steps into *1*—turned and forward to *b*—back, end of *1*—into cage—end of *1*—to end of *2* without pause—into *3*, half to end—turned and forward to *4*—a few steps into *4*—turned, foward to *6*—a pause, then to end of *6*—out and back, via *4*, *n*, *k*, *x*, as far as *c*. A slight pause, then forward to end of *2* again—out, and a few steps into *3*—back and thence to *h*, pausing a moment at *e* to sniff and dig—back to *e*—forward continuously to end of *7* (very slowly by *4*, as if having some recognition of the place, sniffing cautiously from side to side of gallery)—back to *6*—thence forward to C.

Rat *B* up to this time had made little progress. *A* soon came out of C, and for 15 m. the two rats ran about the maze, most of the time in the circuit immediately around the center, *i. e.*, *z-h-x-k-n*, fagging back and forth, digging at the base of the wall and biting at the wires—the depth of stupidity, one would say. *B* reached C 15 m. later, and *A*, a second time, a minute later than *B*.

It is well briefly to notice the points of interest presented in this first experiment. (1) In regard to timidity they presented a contrast with the wild rats of the preceding series. They spent little or no time looking and listening. Their movements were free and unconstrained. (2) Their movements were a trifle less vigorous than those of the brown male. They go about the business more deliberately; their movements appear less automatic. (3) At the turns in the road, the rats frequently stopped, as if in doubt which way to go. (4) The meaning of the sudden stops and turns is not very intelligible. A rat going at a good pace through an unobstructed gallery may stop suddenly or wheel about and go in the opposite direction. This indecisiveness is a constant trait of the rat character, as had been remarked in all my preceding observations and experiments. Other than this

[1] *Amer. Jour. Psy.*, Vol. XI, No. 2, p. 162.

[2] I refer solely to the male in these illustrative examples.

flightiness or unstable attention, no explanation of these movements offers itself. I do not regard this as ultimate; but for the present, at least, it must serve, as the conditions of attention are not apparent. (5) The fortuitousness of the first success is evident. The great number of errors, the repetition of the same errors, the marching and countermarching, and the general appearance of *lostness* amounted to a demonstration of the accidental nature of this first success. (6) It appears also that the very first experience was little profited by. It was noted that *A*, in course of his first wandering journey to C, did seem vaguely to recognize the locality of *4* when he approached it the second time—his slow and cautious movements seeming to indicate that—and there was a suspicion of a similar recognition at 3; but these were the only suggestions of having 'profited by experience,' and after leaving C, *A* wandered just as blindly as *B*, when trying to return.

Most of these points will come up for consideration in the fuller discussion later on.

*Exp. 2.* *A*, 3 m. The two rats kept together until *a'*, having gone to the end of *1*, but having avoided *2* and *3*. *B* paused to pick up a crumb. *A*, forward without pausing, by direct path, *i. e.*, *x-m*, to end of *6*—turned instantly and went like a flash to *7*—thence more slowly to C, pausing but once (to eat a crumb) on the way. *B*, after eating his crumb, went forward slowly and carefully to end of *6*. He looked as though he were following *A's* trail by scent. Here an insane suggestion seemed to grip him (there was no external occasion for fright); he wheeled, made a wild rush, and bit up at *h*. He then ran about foolishly for 15 m, twice going again to the end of *6*. The last time, he flashed out much as *A* had done, not pausing until *r*—thence slowly, as if following a trail. At *s*, *A* and *B* met, stopped a minute to play, then each went his own way, *B* still appearing to follow the trail.

In this second experiment the noticeable things are the immense improvement shown by *A*, and the appearance of "trailing" by *B*. Three possible explanations of *A's* improvement suggest themselves: (1) the right path is selected by sense of smell; (2) by lucky accident; (3) the path is identified by other means than smell. A fourth explanation might be one that included these three as variable factors. I defer discussion of this point, merely indicating that I do not regard the first factor (smell) as important, in spite of the presupposition in its favor, and apparent following of the trail by *B*, noted above.

*Exp. 3.* *A*, 4 m. Distraction on part of the rat prevented a full record. The time of actual performance was not more than 1½ m. For the other 2½ m., *A's* attention was directed to a foreign matter. On the whole, however, he showed more familiarity with the path, making fewer pauses, and moving more rapidly and securely.

*Exp. 4.* *A*, 1¼ m. *B*, a few seconds longer. *A's* course: End of *1*—forward quickly and into *2*—stopped at last turn and back, seeming to recognize his error here—paused at *3*, then to end of that *cul de sac*—turned instantly, and ran swiftly and continuously to *4*—entered *4*, going slowly around circuit *n*, *k*, *x*—paused meditatively at *x*—then suddenly started on a quick gallop, accelerating at *m* and *4*, and not pausing till he reached C. *B* followed about the same course, but went less quickly. He showed less the appearance of "trailing." In the case of both rats, something very like disgust was manifest when they found themselves at the end of a blind alley. The instant recoil, the swift retracing of their steps, and the decisiveness with which they turned from the blind alley into the right path, seldom going *back* now beyond the entrance to the *cul de sac*, seemed to indicate something more of mental content than the mere recognition of the impossibility of get-

ting further that way. Another noticeable fact was the increased security and confidence of all their movements. The slow, blundering *modus operandi* of the first experiment had given place to rapid, definite, purposive movements. This was evident alike in respect to the right moves and in connection with the errors. It has been noted already that the errors were retraced with great rapidity. It remains to remark that in nearly all cases, the entrance into the blind alleys was marked by hesitation, and the journey to the end was made slowly and doubtfully. It was also noted in passing that *A*, after passing *x* the second time accelerated his pace, and increased the acceleration at *m* and *4*, indicating thus a greatly increased familiarity with the path from *x* on.

*Exp. 5.* *B*, 1 m. *A*, 1½ m. Started together. Omitted *1* for the first time. At *2*, separated, *A* entering *2*, *B* going on after a momentary pause. *B* entered *3*—proceeded only a few steps—turned confidently—out and forward to *n*. Delayed here and was overtaken by *A*.[1] Both remained here a little: *B* first recovered his wits, and suddenly dashed back through *4*, and forward to C. Less than 10 sec. from *n* to C; and went with full assurance, hesitating at no point. The appearance of the action was as if some kind of an image of the path to C had flashed in upon the creature's mind touching of simultaneously the motor discharge. At this point I do not wish to discuss the possible nature of the process involved further than to say that the term image does not mean here visual image. Representation, perhaps, would be a less objectionable term to describe this hypothetical mental correlative of the action.

In this experiment *B* for the first time equalled *A's* performance. In all the former tests, both with this apparatus and with that of preceding experiments (*Am. Jour. Psy.*, Vol. XI, No. 2, p. 156 ff.), he had shown a slight inferiority. In the succeeding experiments he was generally first to *C*, though there was slight difference.[2]

*Exp. 6.* *A*, 1½ m. A variation introduced. The entrance into the center, C was barred by Exp. Box II (*Loc. cit.* above) used in a former series of experiments with these rats. The door of this box, which was held closed by strips of paper pasted upon the door and the sill, was opened inward by a spring when the papers were removed. Food inside the box. The reappearance of this problem so unexpectedly and under such different circumstances after the lapse of 27 days constituted an interesting test of the permanence and distinctness of the memory.[3]

*B* first out. Directly to *2*, pause—a very human-like indecision. After 5 or 6 abortive starts each way, finally entered *2* and proceeded slowly to end. Turned and swiftly retraced his steps. At mouth of *2* joined by *A*. Together they proceeded placidly to end of *3*. Turned instantly and galloped back swiftly out of *3*, not slowing up until *e*. Here *B* charmed by the odor from C stopped to dig. *A*, forward soberly, hesitated at *x* turning now right, now left, but finally on to *n*.

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[1] Apropos of the possible rôle played by smell in selecting the path, it is significant that *A*, after following the path traversed by *B* from 3 to 5, *here* diverged, turning into *5* before going to *n*.

[2] Later in this same experiment I observed a striking instance of the difference between the affective character of these rats and of their wild brothers. The laboratory cat jumped upon the maze and showed a friendly interest in the rats. They were startled at first, but soon were trying to bite her toes through the meshes. Under the same circumstances, the wild rats were stiff with terror for 15 minutes.

[3] The term memory is used in this paper in its generic sense. In the present instance it implies: in the presence of accustomed conditions, a mental state leading to the performance of the task incident to the situation. It does not simply "actual and distinct recall;" much less, reference of the reproduction to a former time in the subject's experience. Nor does it imply self-consciousness. It does imply a feeling of recognition—'at-homeness.' The phrase 'permanence of association' might be used, but that this recognition-element is not properly a part of its connotation.

He delayed there a moment—then hastened on to C, via *4*, without further delay or mistake. (*B*, reached *n* before *A* left, but seemed to 'lose his head'—ran back to *e*—to *n* again—then *5*—reached C more than a minute after *A*.) When *A* came to the barricading door of the box, he attacked the paper without hesitation, almost automatically. After pulling off the first paper, he ran back a little way in the characteristic half-frightened manner of the rat, but returned immediately and removed the other paper. All accomplished in a few seconds. The memory was perfect.

This experiment illustrated also the fact of the increasingly definite recognition of critical points, as evidenced by the hesitation and indecision at those points. This was decidedly more striking than in the preceding experiments, especially in the case of *B* at the entrance of *2*, where, as stated, he exhibited a quite human hesitation, turning now one way, now another. The conduct of *A*, at *x*, was hardly less noticeable. *B*'s hesitation continued all the way to the end of *2*, as indicated by his slow and doubtful progress; and his disgust when he reached the end, was as manifest as had been his indecision at the beginning.

The rats were exceedingly active during the evening, traversing curiously all the galleries of the maze, investigating every angle and nook; and returning frequently to C for a drink of milk—they had carried the bread out into the maze at once. As an illustration of the rapidity and accuracy of their movements, I saw both rats go from *n* to C without a suspicion of a pause in less than 5 seconds. The distance is approximately 16 ft. The path includes 16 right angles and 4 chances for error.[1]

*Exp. 7.* *B*, 3 m. Slow in starting. *B* went to end of *1*—then slowly forward as if feeling his way—eliminated errors *2* and *3*, hesitating only slightly—blundered at *x*, going to *n*. After running back and to between *n* and *k* for a moment, he passed on *via x*, *m*, etc., to C, without further hesitation or error. Tore off the papers instantly on reaching C. Memory on this point quite as perfect as *A*'s. After taking a drink of milk, *B* seized a piece of bread and started back—paused at *h* to eat—met here by *A*, and hastened on to *c*—stopped again to eat. *A* followed here in a few moments, and *B* went to the cage. Almost immediately he went again to C and brought back the other piece of bread. The rats ate their supper in the cage, making frequent trips to C for milk. *B* made the journey twice very swiftly without error or delay; and both of them, several times with but one error. They take a run to C for a drink as naturally as they would turn around, or as a man would go out between the acts. The ease of it all indicates a pretty definite knowledge of the way through the maze, as well as of the hedonistic end to be attained by traversing that path.

*Exp. 8.* *B*, 30 sec. *B* made the journey with only one error—went half to end of *1*—and without indecision at any point. (He paused 6 or 7 seconds at one point, but because his attention was caught by something outside of the maze). Tore off the papers instantly. The total time between entering the maze and entering the box was just 30 sec. Deducting the time of the pause (above noted) and that for pulling off the papers—about 5 sec.—we find the actual time in traversing the maze less than 20 sec. This rapidity of movement illustrates—measures roughly—the almost automatic definiteness of the mental pro-

[1] The next morning, I found the rats had carefully deposited the remnants of bread and the *bits of paper* in their cage. A few days later, after I had given them abundant excelsior for a bed, they ceased to save the bits of paper from the box. The collecting impulse seems to be inhibited by the feeling *enough-at-home*.

cess—not quite automatic, as appears from the fact that the animal will again make mistakes and show indecision at critical points. The movements are habitual but not secondary automatic. Attention and discrimination are not wholly shelved. *A* was a few seconds later. Both went back to the cage with their bread without mistake. In making another trip to C a few seconds later, *A* went right to *4;* here hesitated as if "scratching his head," then entered this gallery slowly and doubtfully—only a few steps however; then with a sudden turn and a triumphant flick of his tail he returned to the correct path. This is peculiarly interestiug from the fact that he made this error complete, the first time, going completely around the circuit *4*, *n*, *k*, *x*, *m*. I watched *A* make two more journeys to C. The fourth time he did not pause or hesitate or slacken his pace at *4*. As in Exp. 7, frequent journeys were made to C for milk. These averaged one every 4 or 5 minutes during the half hour I watched. They were made generally without error and in as brief time as 15 or 20 sec.

After the edge of the appetite is worn off a little, the rats tend to let loose the play instinct in the fullest degree. In all their journeys they 'play by the way,' strolling nonchalantly into the blind alleys, now sniffing listlessly, now with half-eager curiosity in all the corners, and angles. That they *know* their way pretty well, however, is evident from the manner in which they take a sudden start from any place in the maze and 'flash' to the end—either end. To one who is familiar with the ways of the white rat, or who is able to imagine his action from the descriptions and figures above, the term 'flash' in this connection will be appreciated as realistically literal.

*Exp. 9. B*, 1 m. (Exp. Box II not used). Each rat made 2 errors: both entered *1; A* took long way at *x*—*i. e.*, *k*, *n*, *4*—*B* turned into *4*, went as far as *n*, then retraced his steps. It is noteworthy that these are the most persistent errors. Neither rat hesitated at *2* or *3*. It is not apparent why the error at *1* is so persistent—it was fully eliminated only once. The persistence of the errors at *x* and *4* is a simpler matter. A glance at the diagram will show that these mistakes may be rectified without a return—indeed they are not, properly speaking, mistakes at all, but rather failures to select the shorter path. In each case the rat has but to push ahead in order to recover the right trail. It is probable that these errors would be fully reduced with a sufficient number of trials—in fact other series of experiments confirm this view, though, as will be seen, they are the last errors to be sloughed off; and, with some rats, absolute certainty is not reached in a great number of trials. In one case, forty did not suffice.

The series really concludes with this experiment, the tenth serving merely to test the memory of the maze experience. It adds little or nothing to the data for explaining the formation of the associative nexus, and the mental material and powers involved in that process.

It was made 22 days after Exp. 9. The results were: *B* reached C in about two m., making errors at *1*, *2*, and *3*; *A*, a few seconds later, errors at *1*, *3* and *x*. They clearly recognized the maze, but some of the details had slipped away. *B* hesitated at both *2* and *3*, as did *A* at *3* and *x*, showing that there was recognition of these critical points, but that the memory was indistinct. After returning from *3*, *B* proceeded more securely—his gait had been slow and doubtful before—hesitating only at *x* and but a second there.

The same experiment in several other cases gave similar results—a constant tendency towards a partial lapse of the association.

## Analyses of Results.

In appreciating the results of this series of experiments,

about the same facts come into view, only more distinctly, as in the case of the wild gray rats; the initial indefiniteness of movement and the fortuitousness of success; the just observable profit from the first experiences; the gradually increasing certainty of knowledge indicated by increase of speed and definiteness, and the recognition of critical points indicated by hesitation and indecision; the lack of imitation and the improbability of following by scent; the outbreak of the instincts of play and curiosity after the edge of appetite is dulled. In addition are to be noted the further observations upon the contrast between the slow and cautious entrance into, and the rapid exit from the blind alleys, after the first few trials; the appearance of disgust on reaching the end of a blind alley; the clear indication of centrally excited sensation (images) of some kind; memory (as I have used the term); the persistence of certain errors; and the almost automatic character of the movements in the later experiments. Viewed objectively, these observations all converge towards one central consideration; the continuous and rapid improvement of the rats in threading the maze, amounting to almost perfect accuracy in the last experiments. No qualification of this view was found necessary in the light of many later experiments. Rather they all confirm it. The mental aspect is considerably more complex, the mental factors, much more difficult of analysis and evaluation; but the central fact in the process seems to be the recognition by the rats of particular parts of the maze. Deferring consideration of this side of the matter, and looking now only at the objective side the important points are, as in case of the wild rats: (1) the increase in speed, and (2) the decrease in the number of errors and in uncertainty. Comparison of the two points is as follows:[1] I-13, 13′. II-3, 2′. III-4, —. IV-1¾, 4′. V-1, 2′. VI-1½, 3′. VII-3, 2′. VIII-½, 1′. IX-1, 2′. (X-2, 2′). As with the wild rats, we find a fairly constant decrease in time and in number of errors. Similarly, there are fluctuations both in time and number of errors, *e. g.*, Exp. 9 shows an increase over Exp. 8 in both respects. This is to be expected from the character of the animal and the confessed impossibility of completely controlling even the external conditions, not to mention the particular internal conditions in each case. Allowing for such variable factors, the relative time required and number of mistakes made furnish a fairly accurate index of the progressive acquaintance of the rats with the problem. The contrast between the first slow, blundering, accidental success and the definitely foreseen success of Exp. 8 (taken as the best) is striking. This is brought out even more saliently by the

[1] Significance of the numerals is the same as *supra*.

graphic representation on p. 201 than by the figures collocated above. The solid line indicates the course followed in Exp. 1; the dotted line that followed in Exp. 8.[1] The arrows mark the point where the rat stopped and turned about, and are pointed in the direction he was headed when he stopped. The dots (•) indicate points where considerable pauses were made. The arrow and dot between *4* and *m* indicate that the rat, when returning from an abortive essay into *4*, went as far as that arrow, paused, then turned and went forward.

Turning now for a brief consideration of the relative results of experiments with the two kinds of rats, some interesting and rather unexpected facts crop out. Comparison of the time and error tables of the two series discloses no considerable superiority on either side, although in this comparison I disregarded entirely the first two experiments of Series I, comparing the 5 trials recorded of Series I with the first 5 of Series II. The average of times required and errors made gives the white rats the advantage in regard to time; the brown rats, in regard to errors.[2] In view of the handicap of two experiments, it will be seen that the advantage lies with the white rats throughout. The brown rats had gained comparatively little from the first two experiments, imperfectly conditioned to be sure, but in which they had the usual freedom of the maze during the entire night. I am of the opinion that even if the conditions of experiment had been identical, the balance still would have tipped in favor of the white rats, so potent is the inhibitive influence of fear with the wild rats. The rational conclusion seems to be that there is little difference between the two in actual intelligence. The wild rat is somewhat more vigorous and active, and consequently this excess of activity increases his chances of accidentally hitting upon the right path. This might secure him a slight advantage *in time* in perfecting his knowledge of the path. It would not signify, however, any advantage in quality or degree of intelligence. It might ensure a larger number of associations (supposing a free life for both animals) but would not make any difference with their delicacy or complexity. In short, the results of superior activity are not intelligence, though they seem often not to be discriminated, in accounts of human as well as animal doings. This superior activity, however, is fully balanced by the wild rats' greater susceptibility to fear under these strange conditions.

[1] That Exp. 1 gives the record of Rat *A* and Exp. 8, of Rat *B* is inconsequential. The attainments of the two were practically equal. (The course was marked on a diagram at the time of observation.)

[2] Average time: Series I, 5 m. 19 s.; Series II, 4 m. 33 s. Average of errors: Series I, 4 1-5; Series II, 5 1-5. In Exp. 3 of Series II, I was not sure whether there were 4 or 5 errors. The maximum is taken.

In ability to profit by experience—in this case, to learn a definite route involving possibilities of frequent error—the two are not far different.

This conclusion does not tally very well with the general opinion that animals suffer mental deterioration under domestication. However that may be with other animals, it evidently is doubtful in this instance. The white rat in comparison with his wild congener is somewhat less vigorous and hardy (especially does not endure cold or hunger so well), and has sloughed off some of the timidity and suspiciousness of the wild rat; on the other hand, his senses with the exception of sight are as keen, his characteristic rat traits are as persistent, and his mental adaptability is as considerable. The *modus operandi* of the two kinds in the maze shows little variation. Likewise there is no difference in the curiosity manifested, either in kind or degree. In view of the many generations of luxurious idleness[1] of the white rat, this profound and enduring nature of specific psychic traits is striking. A pertinent illustration was furnished by a young rat that escaped from his cage and was loose about the laboratory for several days. He had just been weaned when the accident occurred. Food was rather scarce and he got pretty hungry. Finally one morning he found his way into the chicken pen, and in less than two minutes had killed two chickens, and was upon a third when discovered. The chickens were three times as large as himself. The killing was done by biting through the throat of the victim, and was as neatly and deftly executed as if the executioner were an old hand. The importance of this illustration lies in the fact, that this is exactly the method of killing employed by the wild rats. The only possible preparation in his own experience this pigmy could have had for such serious business must have been in play with his fellows. That, however, was general rather than specific; and, at best, was of slight importance, as he had reached the playing age but few days before. Another typical illustration of the persistence of specific traits is furnished by a perfectly tame pet rat that exhibited the greatest fondness for a hole in the base-board of a room where he was allowed to run, making for the hole every time he was set free in the room, and dodging in and out at every sound; yet so tame that he would come out and allow himself to be caught as often as I went to the hole and called. Such cases give some suggestion of the tenacity of those fundamental specific traits which "persist with undiminished vigor" long after the conditions of life which

[1] I cannot find that the white rat is known to exist in the wild state. There is a tradition that it was brought to the Occident from an immemorial existence in China. I am not able to verify this tradition—or disprove it.

called them into being have changed radically. In these cases non-use certainly has had but little effect in reducing the potential force of specific instincts.

What is true of such relatively superficial specific traits is doubly true of generic instinct-feelings. Curiosity is a good example, frequently coming into evidence as it does in these experiments. Its intensity is not diminished with the long domesticated white rats. Nor is it greatly changed. Ribot, rightly regards curiosity as the basis of the 'intellectual sentiment.' "This primitive craving—the craving for knowledge—under its instinctive form is called curiosity. It exists in all degrees, from the animal which touches or smells an unknown object, to the all-examining, all-embracing scrutiny of a Gœthe; it always remains identical with itself."[1] I am persuaded that Ribot is right in regarding this affection as primitive and as a primitive craving for knowledge; not merely a reaction to hunger or sex stimulus. The desire for familiar acquaintance with environment, concomitant with fear and uneasiness in strange surroundings, is about as fundamental as hunger. Observers of wild animals in their native haunts tell the same tale. The astronomer who orients himself with respect to infinite worlds satisfies the same craving for knowledge and calms the same uneasiness in strangeness as does the animal which seeks all the knowledge possible to him of his universe. The reduction of chaos to cosmos begins there.

Such considerations as these suggest the question whether zoölogical psychology may not profitably turn from its almost exclusive search for variation, to a search for the relatively invariable factors in the animal mind. In order to do this the ideals of structural psychology must be departed from somewhat, and attention directed to the study of the instinctive traits and tendencies, out of which, in higher differentiations, human nature is made. From the point of view of psychic statics these are composite and analyzable; but, from the point of view of psychic dynamics, they are themselves primitive and elementary. By this method, if by any, will be gathered the material for a natural history of mind. From this source light may be expected upon many obscure problems in individual and anthropological psychology.

### General Form of Animal Intelligence.

The amount and variety of fact brought out in the preceding description and analysis must serve as excuse for this somewhat tedious presentation of details. It justifies the initial assumption that the results of such a study as this are qualitative

[1] Ribot: Psy. of Emotions. (Eng. Tr.) p. 368.

rather than quantitative; and that a generalized statement of results or any graphic representation whatever of the data by curves, would indicate only the most general form or tendency of animal intelligence, which was decidedly less what I wished to exhibit than the details of the performance. My conviction of the importance of this aspect of the case is strengthened by M. Hachet-Souplet's brilliant suggestion of a new method of classification of species from the psychological point of view.[1] M. Hachet-Souplet shows clearly that there is very good reason for cutting loose from the trammels of morphological classification in our psychological investigations of animals. If, however, such a desideratum is to be realized, it must be by studying the mentality of the different kinds of animals with the same minuteness that morphology employs in its domain.

A few words, however, as to the 'general form of animal intelligence' and a more adequate appreciation of the value and limitations of the 'curve' in connection with the same may not be *mal apropos*. In a former paper,[2] I have noted that a time curve would be an insufficient index of the definiteness and certainty of an animal's mental processes, on account of the inconstancy of internal conditions. In this paper I have pointed out that a more adequate representation might be made (at least for these experiments) by compounding the time and error curves, and also a curve representing the indecision at critical points—if such could be extracted. Such a compound curve, however, would still be far from telling the tale fully and precisely.

The one extensive and important study in comparative psychology in which the graphic method of presenting results is largely employed is that of Dr. Edward Thorndike upon cats, dogs, and chickens.[3] Dr. Thorndike's methods with his cats and dogs was to confine the hungry animals in boxes, small enough to be uncomfortable, from which they might escape by "some simple act, such as pulling at a loop of cord, pressing a lever, or stepping on a platform." Food was exhibited outside

[1] P. Hachet-Souplet: Examen psychologique des animaux, Paris, 1900. Schleicher Frères.

[2] *Am. Jour. Psy.*, Vol. XI, No. 2, p. 136.

[3] Edward Thorndike: Animal Intelligence. Psy. Rev. Monograph, Vol. II, No. 4, June, 1898. Dr. Thorndike's methods have been criticised by: W. Mills (Psy. Rev., May, 1899); C. Lloyd Morgan (Nature, July 14, 1898); and Kline (*Am. Jour. Psy.*, Vol. X, Nos. 1, 2, 3). Kline's criticisms are fragmentary, but valuable. Morgan's Review in Nature is discriminating and sympathetic. Mills raises some objections to Dr. Thordike's work, but is blinded at times to the other's meaning by his polemical ardor. Thorndike has replied to Mills (Psy. Rev., July, 1899).

the enclosure as a special incitement to vigorous effort; and escape was rewarded with the food. The time required to escape in successive experiments was recorded, and the results represented by curves. These time curves are regarded by Dr. Thorndike as exact indices of the progress of the animal in the formation of the required associations. Though these temporal data do not seem so significant to me, as to Dr. Thorndike, for the reasons given above, yet they do exhibit, I think, what I have called the general form of animal intelligence; or as Professor Morgan expresses it in an analysis of Dr. Thorndike's paper (v. note above), "they bear out the contention that the method of animal intelligence is to profit by chance experience, and depends upon the gradual establishment of direct associations." I suppose Höffding means essentially the same thing when he says: "The simple primitive consciousness does not feel the need of concepts but goes passive from disappointment to disappointment." Professor Morgan reports in the same paper, having attempted to extract from some of Mr. Thorndike's carefully plotted data, a mean curve for the method of trial and error. The attempt was not very successful he admits; but he thinks the resultant curve does indicate the gradualness of the process which theoretically would be expected. On the whole, this conclusion seems to me essentially sound. At least, my own observations with rats, under what seem to me more natural, and sympathetic conditions, give confirmatory results. I have plotted tentatively a number of time curves, both from the results of my experiments with the maze and with other devices; and they show no radical variation from the general form of Dr. Thorndike's curves. I think Morgan is right in asserting that "the form of his curves affords no particle of evidenee for reasoned behavior." No more do mine. It must be remembered that Morgan rigidly limits the term reasoning or rational procedure to the process of drawing logical inference. The relation between experiences must be perceived as such. The transitive moment between focal points in consciousness must itself be capable of becoming focal. The reasoning creature must be able to 'focus the therefore, think the why.'[1] In the broader sense of practical adaptation to varying conditions by direct association; or as Binet[2] defines it: "an organization of images determined by the properties of the images themselves, so that the images have merely to be brought together for them to become organized, and reasoning follow with the inevitable necessity of a reflex"—Morgan readily admits reasoning in animals.

[1] Morgan: Comparative Psychology. *In locis.*
[2] Psychology of Reasoning. (Eng. Tr., p. 3.)

Morgan further made a comparison of his mean trial and error curve, with a mean curve of rational procedure, which he plotted from Dr. Lindley's data compiled in his "Study of Puzzles." (*Am. Jour. Psy.*, Vol. VIII, No. 4.) These present a clear contrast, he thinks. In the latter case he finds "a sudden leap from failure to success when the trick of the puzzle was discovered and understood;" as opposed to a "gradual sweep towards rapid and assured success" with the former.

There is one fact, however, in connection with the trial and error curves that Morgan does not remark. Nearly all of Dr. Thorndike's curves show a sudden fall after the first success. My own experiments with various devices gave similar results. This fall is analogous with the fall in the curve of rational procedure after the 'trick was understood.' This fact does not affect the distinction between the two processes. The sudden fall in the trial and error curve indicates only effectiveness for reproduction of the first right association. In this method, learning the trick or the task, depends in the first instance upon performing it fortuitously so far as previsioned end is concerned; and requires then time and repetition for perfecting the knowledge. Improvement depends upon memory of previous performances, or forgetting useless details. The relation of the acts does not become focal. On the other hand, in rational procedure, the trick may be understood before it is performed. The entire plan of solution may be envisaged before a move is made. As a matter of fact, however, in the great majority of cases tested by Dr. Lindley, in which rational procedure was employed, the understanding at the beginning was only partial, the plan was vague and hazy. The understanding became full and definite through abortive attempts at practical solution. The younger children, Dr. Lindley found, as would be expected, used almost exclusively the trial and error method, chance success and direct association. With older children rational procedure based upon a considered plan came progressively into evidence. "The younger children succeed through a long series of slight variations. Occasional lapses into useless movements occur; but the trend is by a slow and primitive method of exclusion towards the goal." Among older persons the inhibitive influence of failure is stronger. The memory of the failure takes its place as a substantive element in consciousness and constrains the subject to reconsider, to deliberate. The variations, too, are wider and more far-reaching, indicating a larger and more complex grasp of attention. Yet comparatively few, even of adults, pursue a strictly rational procedure. The close similarity between animals and children is obvious.

The fact that the trial and error method plays so large a part in human mentality, and especially that it predominates with

children, still further supports the view taken in the beginning of this paper that animal intelligence works almost exclusively by this method. Although this hypothesis was assumed for the present case only, it certainly covers adequately a very large part of animal activity. Most anecdotal cases of animal reasoning are explainable upon this ground. So far as the narrowly experimental studies of animals go, they point in the same direction. As yet, however, these are too limited in number and too restricted in scope to be very conclusive.

On the other hand, however, there is yet something to be said. The analogy between children and animals is so close that the manifestation of reason in some very young children suggests that some animals may have the same power. Preyer cites the case of a two-year-old child getting a cricket and standing upon it in order to reach a desired object.[1] M. Hachet-Souplet[2] shows wide variations of psychic faculty among animals of closely related morphological groups. Individual variations, too, are indubitable. M. Hachet-Souplet has no doubt that some animals reason. His opinion is important since he is an experimenter in a large and fruitful way, and is at the same time in the main sharply critical of his facts. He describes a striking case of reasoning in a coati, as follows: "Un de nos amis nous ayant rapporté un merveilleux trait d'intelligence de la part d'un coati, nous avons résolu de provoquer artificiellement autour d'un autre coati, des circonstances analogue à celles dans lesquelles le premier s'était trouvé, quand il donna une si grande preuve de sagacité. On sait que l'espèce est très friande d'œufs de poule; nous en plaçâmes un sur une haute cheminée de façon à ce qu'l pût être vu du coati et, après avoir éloigné légèrement les sièges, nous quittâmes la pièce; en nous arrangeant toutefois de manière à ne rien perdre de ce que ferait notre sujet.

"Il s'agita d'abord, saute deux ou trois fois; mais, voyant que son élan ne le portait qu'à mi-hauteur de la tablette, il semblait réfléchir un instant. Il se dirigea ensuite vers une chaise en chêne ciré qu'il essaya d'attirer du côté de la cheminée, mais ses pattes glissait sur le bois et il renonça à son enterprise; il semblait désespéré. Cependant il aperçut dans un coin un paquet de vieux chiffon et parut frappé d'une véritable *idée. Ayant pris une des bandelettes, il en entoura le pied de la chaise et il se mit à l'attirer à reculons.* Quand le siège fut contre la cheminée, en deux bonds, mon coati monta sur celle-ci et s'empara de l'œuf." Manifestly the coati in this case perceives relations, more complicated relations, indeed, than the

[1] Preyer: Infant Mind. (Eng. Tr., p. 185.)
[2] *Loc. cit.*

relation perceived by the child cited above. Such a mental expression goes beyond the explanatory possibilities of 'direct association by trial and error methods.'

Perhaps the real question after all is not whether animals perceive relations, but, rather, *what animals perceive what kind of relations.* This would seem to be the logic of M. Hachet-Souplet's position; and, what is more important, it is the logical method of psychogenetic study. Granting that animals may perceive relations, we have, then, definite and feasible problems to investigate: the kind and complexity of the relations perceived. This method should give data for a better understanding of the nature of animal reasoning,[1] judgment, inference, and the consciousness attending these processes. It would not be surprising if degrees of complication and of symbolism were found to constitute the differentiæ.

On the whole, the modern studies of comparative psychology confirm Hume's acute observations on the nature of animal intelligence. "Animals, as well as men, learn many things from experience, and infer that the same events will always follow the same causes. By this principle they become acquainted with the more obvious properties of external objects, and gradually, from their birth, treasure up a knowledge of the nature of fire, water, earth, stones, heights, depths, etc., and of the effects which result from their operation. The ignorance and inexperience of the young are here plainly distinguishable from the cunning and sagacity of the old who have learned from long experience to avoid what hurt them, and to pursue what gave ease or pleasure. . . . An old greyhound will trust the more fatiguing part of a chase to the younger, and will place himself so as to meet the hare in her doubles; *nor are the conjectures which he forms on this occasion founded in anything but his observation and experience.* Animals, therefore, are not guided in these inferences by reasoning, neither are children, neither are the generality of mankind in their ordinary actions and conclusions, neither are the philosophers themselves. Animals undoubtedly owe a large part of their knowledge to what we call instinct. *But the experimental reasoning itself, which we possess in common with beasts is nothing but a species of instinct or mechanical power that acts in us unknown to ourselves.*" This statement of Hume's[2] as to the general form of animal intelligence has not been improved upon. His expression 'experi-

[1] Benn (Gk. Philosophers, I, p. 381,) makes the interesting suggestion that animals reason disjunctively ("after a canine fashion"). It is from the disjunctive form that all other forms of reasoning are successively evolved.

[2] Hume: An Enquiry concerning Human Understanding. Section IX, pp. 85 ff.

mental reasoning' seems to me singularly happy and accurate, guarded, as it is, by the suggestion of its subconscious character —especially happy in comparison with the clumsy expressions of some modern comparative psychologists whose abhorrence of anthropomorphism leads to the opposite extreme both in thought and expression; and causes them to inveigh against that vice *ad nauseam*. I suppose Hume would be dealt with summarily on this score by these writers. But is not a certain amount of chastened anthropomorphism a wholesome specific, a kind of saving grace against the scientific pedantry that thinks to create a new science of comparative psychology with the imperfect instruments of experiment and the law of parsimony. The law of parsimony is important, no doubt, but it may be employed too rigorously. The real difficulty lies not in the tendency to interpret animal intelligence in the terms of human experience, for we have no other way; but in the faulty and imperfect analysis of human experience. That is the real vice of Romanes' work. His analysis did not go much deeper than the discursive adult human understanding. This difficulty is intensified, not only by the fact that human consciousness is permeated, and, as it were, recreated by self-consciousness; but also by the fact, not always heeded, that the more elementary and obscure phases of human experience, as yet, have not been fully and definitely analyzed.

Another difficulty, not less real and important, but not sufficiently remarked, is found in psychophysical limitations. This difficulty is frequently met with in human psychology. Galton's Academicians who regarded mental imagery as 'moonshine' illustrate the point. They experienced no mental images; therefore, mental images did not exist. A similar limitation, Ribot thinks, leads psychologists to cavil at memory of emotions. Now, doubtless, the psychophysical disparities between man and brute are inconceivably greater than between man and man. The immensely greater rôle played by smell, for example, or motor experience, in the economy of the animal mind cannot be fully appreciated. We can hardly have any idea of the radically different tone and feeling of such consciousness; and we cannot say with any precision what modifications of intelligent processes are concealed from our view in this way.

## The Process of Learning.

Whether the process of learning the way through this maze is adequately described as a gradual establishment of direct associations by profiting by chance experience depends upon the meaning attached to the phrase 'profiting by chance experience.' I wish in the remainder of this paper to attempt an analysis of the mental factors involved in the animal's solution of the

problem; and to offer some suggestions upon the character of the perfected knowledge.

It will be well to résumé the facts by following the rats *A* and *B* of Series II from their introduction to the maze, throughout their experience of getting acquainted with their new environment, to the time when they have perfect mastery of the situation. In their first trial, after a lapse of 13 minutes and after many errors, returns, and delays, they find their way into C. Here they are rewarded for their labors by all the pleasure possible to a rat from the satisfaction of a keen-edged appetite by a good meal of bread and milk. This first success is assumed to be accidental; its realization does not depend at all upon previsioning intelligence. The animal does not foresee the end and set to work to attain that end. There is no reflection. The determining conditions in the rat's mind are more immediate in their effect. The most obvious of these are: hunger, perception of the odor of the food, curiosity, normal activity (the obverse of curiosity) and the instinctive special trait of following out tortuous passages—a definite rat-hole consciousness that acts, as it were, thygmotactically. These factors, with the inhibitive balance-spring of timidity, firmly rooted and deeply toned emotionally, constitute the relatively stable background of consciousness over which play the lights of the perceptive and discriminative processes as the animal proceeds with the task. The rat, when he enters the maze, is psychically a confused complexus of these factors. No one of them looms high above the others in the wave of consciousness. Attention is dispersed; perhaps, better, distraction prevails. Nevertheless, 'experimental reasoning' begins at once. The animal keeps constantly moving; but his activity at this stage is evidently sensori-motor (or organo-motor). Motive in the sense of ideated end is absent. The nearest approach to this is the possible idea of a definite kind of food incident to the perception of the familiar food-odor. This is not impossible. On the other hand the effect on consciousness may be only an intensification of the hunger psychosis resulting in an increase of motor activity. However this may be it probably is the animal's instinctive fondness for following out devious ways his, thygmotactic rat-hole psychosis, rather than the smell of the food that gives determinateness to his movements at first. Were this trait less imperative, the rat, when he comes near the food (*e. g.*, at *e*) would become the victim of his hunger and his perception of the position of the food—for the food at this point could be located by smell—and would spend himself stupidly in endeavoring to force an entrance. In general, however, he soon passes on, going directly away from the Canaan of his desire.[1] Failure to get through by gnawing and digging,

[1] Occasionally one does just this stupid thing. Cases were noted in passing.

quickly results in a wavering and dispersion of attention. Concomitantly the perception of the odor relapses into the margin of consciousness, and the instinctive motor tendency at this juncture reasserts itself as the focal and directive influence.[1] The first success then may be set down as the accidental issue of a trial and error series, motivated by hunger and curiosity, mediated by the sense of smell and, more largely, by this instinctive motor trait, and consummated by the pleasure of hunger satisfied. And yet the term *accidental* must be used with reservations. The rats of this series, and of all others in their first trial, seemed to profit at once by experience. By this I mean that after they had made an error once or twice, though they had not yet succeeded in reaching C, they would hesitate or even avoid the error when going over the ground a second time. For example, rat *A* went only a few steps into *3* the second time he reached that place, and avoided *6* and *7* completely. A glance at the record of the first experiment will make this perfectly clear. Such cases may be attributed to pure chance; the conduct of the rat, his hesitation more than his avoidance of the error, indicates, rather, recognition and selection.

It will be remembered that the rats have the entire night each time for exploration of the maze. This results in remarkable improvement in the second trial. In the succeeding experiments the improvement is continuous in the elimination of errors and in the increase in definiteness and speed. The rats soon acquire a practically perfect knowledge of the maze, so that they can make the journey quickly and accurately when they want to do so, or stroll about as they list.

How explain this improvement? What does 'profiting by chance experience' mean in this instance? how is it assimilated and how utilized?

Doubtless one factor in the process is the memory of the pleasant experience at the end. In addition to the undirected and undifferentiated motive of hunger and the motor trait of the first trial, there is, in the second, a dimly ideated end which probably becomes progressively clearer in the subsequent experience. But the essential point is certainly the recognition of the critical points along the way and the discrimination of

[1] With several subjects the odor stimulus was done away with entirely to see whether they would make the first journey to C as well as the rats that had that stimulus. No appreciable difference was shown either in time or number of errors. The rats followed out the maze to the end, C, just as perseveringly as if the food had been there dispensing its savory solicitations. The expected did happen, however, in respect to learning the direct way to C. They made little progress in five or six trials. As there was no pleasant association at the end of the journey there was nothing to determine the building up of this definite association train.

the divergent paths at these points, leading to purposive selection of the right path. The memory of the pleasant experience at the end would be of slight avail, if the rats did not recognize the critical points and discriminate and select their paths. The animal begins by going right and wrong wholly by chance. After a few trials he comes to recognize the doubtful places, and hesitates when he comes to them, undecided which way to take. The external signs of indecision vary between standing still as if trying to think which way to go, and abortive starts each way. Sometimes to these is added standing up and sniffing in the usual manner of orientation. This movement seldom was observed after the first two or three experiments, *i. e.*, after the dilemma began to be clearly felt. At this stage, the choice of path is still about as often wrong as right. The distinguishing accidentia are acquired gradually. Progress in discrimination is marked by decrease of hesitation and in more frequent choice of the right path. The path chosen often is pursued doubtfully. If the wrong one is chosen, the error frequently is retrieved after a few steps; if it is followed to the end the return is made swiftly and the right path is taken confidently. In the final stage, errors and hesitation drop out entirely. The right path is followed from start to finish without attention to specific points *en route*.

It should be noted that the learning was slowest in connection with *o*, *x*, and *4*. The persistent confusion at *o* is attributable probably to its being at the entrance of the maze. At this point there is a maximum of affective excitement. The momentum of association has to be gathered as the animal goes along in the familiar path. In a remote way it may be likened to the stumbling and groping of an orator at the beginning of a familiar theme. The suggestion that the rats might have a penchant for right as opposed to left was found baseless by the use of the reversed maze. A definite memory of direction seems to be required. The slower discrimination at *x* and *4* was due doubtless to the fact that wrong choice at these points consisted in taking the roundabout, rather than the direct path. Strictly speaking there was no error. In all the other cases, taking one path was associated ultimately with success; taking the other, with failure—disappointment. In these cases the association would seem to be between path and distance.

In such cases profiting by experience manifestly involves the processes of recognition, discrimination and choice. If the problem set were merely the selection of one effective movement out of several haphazard movements, as was the case with the puzzle-box experiments reported by Dr. Thorndike[1] and my-

[1] *Loc. cit.*

self,[1] then the profiting by experience could be accounted for by the fading away of the useless movements. They would drop off like dead branches from a tree, of their own weight. They would be associated with nothing—either positively or negatively. The right movement would be selected *naturally*. In the present case, however, two direct associations are formed and discriminated between, and the advantageous one selected. Recognition of the critical places is equivalent to doubt as to the right path. This doubt is the correlative in consciousness of the struggle between the two associations or 'constructs.'[2] The positively useless or the less advantageous association does not fall away mechanically, but only in virtue of discrimination between the two constructs, and, finally, the conscious selection of the right one. In such a case as that of choice at $x$ if the animal did not consciously select, there could never be any fixed association; consequently never any habitual reaction. Both ways lead to success. In a sufficient number of trials the theory of probabilities would require an equal number of selections of each path. But the short road is soon habitually selected, just as is the right path at other critical points. There is involved an elementary form of comparison and judgment; for comparison, judgment and reflection, even, are present in embryo. They all take their rise in the struggle of ideas and images, and lower down of 'constructs,' which "gives in animal, as in man, the illusion of choice and free intelligence."

### Modalities of Sensation.

This section is an attempt to appreciate the rôle of the different sense-modalities in learning the task.

The conditions of the experiment were such as to exclude any very direct influence of taste and hearing. Taste gives only a pleasurably-toned experience at the end, the significance of which has been noted. The influence of hearing is limited to occasioning affective variations. Neither gives any data for solving particular difficulties.

*Smell.* The sense of smell might be supposed, *à priori*, to play the leading rôle, but in the present case its claims to primacy are doubtful.

In the preceding section it has been shown that the location of the food by odor, and hence the end to be reached, was an unimportant factor. In fact, it is improbable that olfactory sensation *per se* has much greater spatial significance with animals than with man. In general, animals perceive direction of odors only with the aid of air currents. The perception is quite as much tactual as olfactory.

[1] *Loc. cit.* [2] *Cf.* Morgan: Comparative Psychology.

It is even clearer that the trail of the first accidental success was not followed subsequently by scent. In the first trial the rats invariably traversed practically all the galleries; and, after appeasing their hunger a little, carefully investigated the entire maze. It would be impossible, therefore, for them to select the right path by scenting the trail. Again, the second rat frequently turned aside from the route marked out by his immediate predecessor; either he was not following the trail or he could not discriminate the fresh trail from one a day old. Further, the recognition of critical points, and the fact that the rats frequently ran long distances with heads up—*e. g.*, when carrying food—are evidence against the supposition. These facts together are sufficient to throw the theory out of court.

The conclusion is drawn for the present case only. It is perfectly apparent that animals of this class do follow trails by scent in the right circumstances. These facts point to the complexity and variety of the animal mind, and are a warning against naïvely accepting 'simplest explanations.'

Another possibility in regard to smell is that particular points in the maze may have been associated with definite peculiarities of odor. The constant sniffing and extensive olfactory investigations of the rats lend color to this thought. The experience thus acquired, may, however, influence only the affective tone—connect directly with the emotional tendencies which determine the animal's conduct. Such a relation is indubitable. I found, for example, that putting rats perfectly familiar with the task, into a new maze, differing from the one learned only in *newness*, threw them into extreme emotional excitement. They acted as though the task were absolutely new to them. They were curious, timid, and hesitant; errors were as frequent as in their introductory trial. After finding the food, they continued eagerly exploring and re-exploring the maze. As soon, however, as they had become familiar with the new odors, their former facility returned; they made the journey as quickly and accurately as before. This would not have occurred, had it been necessary for them to establish a new series of smell-position-direction associations. The inference is clear that the effect of smell sensations is general and emotional, rather than that delicate and discrete associations of odors with special positions are set up. The point, however, is not absolutely secure. Probably more conclusive evidence might be obtained by testing rats with olfactory nerves paralyzed.

*Sight.* Sight is much less relied upon, and, relatively, much less acute than smell and hearing—the psychic organs respectively of food-getting and defence. This corresponds with the poor development of the eye and optic nerve.[1]

[1] The eye of the wild brown rat is better developed than that of the white rat, but the two rats varied slightly in their conduct in the maze.

Several tests were made, the results of which indicated that visual perception played no part in the processes of recognition and discrimination.

1. It was suggested to me that the direction of light, by analogy with Lubbock's[1] experience with ants, might be a factor in the chance of path. Lubbock found that "when the direction of the light was changed, but everything else left as before, out of seven ants, five were deceived and went in the wrong direction." (This was after they had learned their path perfectly, of course.) Fortified by further experimentation, Lubbock concludes that "in determining their course the ants are greatly influenced by the direction of the light."

As the rats did most of their exploring in the dark, and as the brightness element is only one factor in the visual datum, not the total datum as with the insect, it was improbable that this factor should be very influential. Nevertheless, it was made a matter of experiment. Tests were made by having the rats learn the path perfectly with the direction of the light constant. The light was then transferred to the opposite side for a few trials; after which, it was alternated at unequal, though frequent, intervals. The results were: (1) In most cases, change of direction of the light seemed to produce a very slight effect upon certainty and celerity of movement; but hardly more than might occur as normal variations under constant conditions. (2) Some subjects showed absolutely no effect. (3) After the first change the alternation produced no effect. This shows that the effect when it occurred was merely a slight affective disturbance—a retardation, not a change of the cognitive process. Plainly this is a very minor factor.

2. A partial test of the part played by sight in the recognition and discrimination necessary to the formation of special associations at critical points was made as follows: At all such points, bright red posts, ¼ inch diameter, were placed in the middle of the right path a few inches beyond the dividing of the ways. When the rats had learned the path perfectly the posts were removed. Two rats only were tried, the results being *nil*. These rats did not learn the path more quickly; nor did they exhibit the slightest variation in conduct after the posts were removed. It is tolerably clear that visual data, if effective, must be of a more general character. The animal does not hang his association upon a gross and obvious object.[2]

3. Another method of partial experiment suggesting itself was to blindfold the rat after he had learned the path. The evident

[1] Lubbock: Ants, Bees, and Wasps, p. 267 ff. (6th Ed.).

[2] It is improbable that the action had become so habitual as to dispense with what was at first a determining factor in the formation of the association.

objection to this plan is that it would change completely the conditions of attention and emotion. If the subject blundered and failed nothing would be proved. If he did his work about as well as before there would be a negative demonstration of the slight importance of visual perceptions; *not, however, of visual sensation*, for its effect might still be present as visual images co-operating in the mental process.

Fortunately, nature stepped in and performed a conclusive experiment for me. A number of my rats came to me with diseased eyes. Before I discovered this, two of them, an adult male, *X*, and a young female (about 10 weeks old), Y, had become blind. I had already started them learning the maze, with two others, when I noticed their blindness. After the fifth experiment they were totally blind. In the first two experiments distinct impressions—if white rats have such—may have been possible to *X*, and brightness sensation until the fifth. Rat *Y* may have had brightness sensations in the first two experiments, but not later. At this time the general health, vigor and temperament of these rats were unaffected by their malady.

The results of the experiments with these blind rats were so striking that I give them somewhat fully. Until after the ninth trial, the two normal rats were continued with the blind ones. They were then removed, and thirteen more experiments were made with the blind ones. Following that, the latter were tried in the reversed maze. The blind rats learned the original task as well as the normals—all the normals experimented with. Rat *X* in this case learned the path before either of his normal companions. In Exps. 5 and 6 he was first to C, and made fewest errors. In Exp. 7 he made the round in 50 seconds, without error, and with slight hesitation at two points only. In the succeeding 15 experiments he showed practically perfect acquaintance, though occasionally making errors. His conduct in the maze did not differ materially from that of normal rats. He ran in the middle of the galleries, rounded the corners quickly and precisely, and carried on the usual investigations. At critical points there were the same hesitation and indecision manifested as with the normals, by alternately turning each way as if stayed in the grasp of conflicting images. Occasionally he would nose along the several sides before starting on again. This probably was not a direct means of ascertaining the way, for, later, I cut off his feelers, also those of some normals, without any effect upon their ability to find their way.

The results with Rat *Y* were even more interesting, as she was certainly totally blind after Exp. 2. She was somewhat longer in learning the way than *X* and the normals. At

first she was slow and diffident in starting, and less facile in getting about; she ran somewhat gropingly, and frequently almost bumped into the ends of the galleries. These defects soon wore off, and she kept her path and rounded the corners as nicely as the others. Certain errors, however, clung persistently, notably at *2* and *3*. After Exp. 3, she had little difficulty beyond *3*. Until Exp. 11, she went each time to the end of *2* and *3* as mechanically as if these were essential stages of the journey. I began to wonder whether the habit was so firmly fixed as to defy the benefit of chance experience. In Exp. 12, however, *2* was dropped out; in Exp. 13, *3* was dropped, but *2* reinstated. In Exp. 15, *2* and *3* were finally eliminated, and did not reappear. In the last 7 experiments she made no errors and seldom hesitated or showed indecision. This blind rat thus eliminated errors that had become almost automatically habitual. The experiments with the reversed maze[1] gave the only suggestion of importance of the visual factor. The blind rats when first put into the reversed maze were more disoriented and confused than the normals. Not until Exp. 3 did *X* succeed in getting to ♀.[2] The normals, on the contrary, seemed to have profited by their experience with the other maze. They made better time, fewer errors, and showed less indecision than in the first experiment in Maze I. They fell off badly, however, in the second trial. The discrepancy between the blind and the normals quickly disappeared; in Exp. 5 the blind rat did as well as the normals. So also, in the successive alternations of the mazes, the blind rat perfected his distinct knowledges of the two as quickly as the normals.

This slight superiority of the normal rats, however, does not seem to me to mean that visual data exerted a determining influence. Rather, with the blind rats the motor element was so exclusive in the reproductive process as to make readaptation proportionately more difficult. With the normals sight, though probably contributing no determining data, served to distract attention from the established reproduction. Sight certainly is not a *sine qua non* in the process of experimental reasoning incident to these experiments. Its service is superficial, and may be dispensed with almost without loss. Its office in the essential processes of recognition and discrimination is hardly

[1] The method of experimentation was to transfer the subject, after he had mastered the original maze, to the reversed form—*i. e.*, right and left interchanged. After this reversed form was learned the rat was returned to Maze I. The mazes then were alternated till both were learned perfectly.

[2] Rat *Y* died in Exp. 3, so these data are from observations of Rat *X*.

appreciable. Its forces are deployed in the background of consciousness; they do not get into the forefront of action.

By this process of elimination the conclusion emerges that the tactual motor sensations furnish the essential data for the recognition and discrimination involved in forming the special associations at critical points. How the animal recognizes critical points it is impossible to say. The most reasonable supposition is that in the gradual formation of the motor memory of the entire course, at the established distance-intervals, the conflicting images of turning in one direction or the other spontaneously arise, resulting in indecision—the sign of recognition. There is some positive evidence in support of a distance quality in the animal's image to be remarked below. The machinery of discrimination seems tolerably clear. In any given case it consists of direct association of *the motor image of turning in one direction* with success, and the motor image of turning in the other direction, with failure. This is the explanation in the cases where one alternative is a blind alley. In the cases where the alternatives are longer and shorter we have to suppose an association between direction and distance; between turning in one direction and the distance traversed or *the time consumed.* Perhaps the quality is temporal rather than spatial; or indeed it may be that the temporal and the spatial *qualia* of this modality of sense are undifferentiated in the lower animals. The positive evidence of the distance quality noted above appears in the fact that the rats quickly adopt the shorter road where there is choice of longer or shorter. It has been pointed out that all the rats experimented with learned, sooner or later, to take the shorter road at *x*. Inasmuch as both roads lead to Rome it is difficult to see why the shorter one invariably should be selected, unless it is known as shorter, or, in other words, unless quicker satisfaction is associated with this path than with the other. It is equally difficult to see in what terms this association could be mediated other than those suggested—a distance or temporal idea in tactual-motor terms.

The fact of the invariable adoption of the 'shorter circuit' was brought out more clearly and forcibly by a special test. A normal and the blind rat *X* were used. Both had been familiar with both mazes (direct and reversed) for weeks. Their knowledge was as nearly automatic as possible. A path was opened, then, between *d* and *h*, by cutting the walls at *w* and *z*. A large part of the first (left) half of the journey thus was cut out.

In the first trial (normal rat) the rat went automatically *via* the old route—paid no attention to the new one. He likewise went directly by the opening at *z* when he came up from *e*. In the next trial, however, he took the new path unhesitatingly

through *w* and *z*, and turned correctly—*i. e.*, to the right—at *h*. In the third trial he took the new path, turned correctly at *h*, then paused and went half way to *e*; paused here again, made several abortive movements each way, but finally turned correctly and went forward to C with confidence and speed. In the next two trials the right association became pretty well fixed; the new route was learned perfectly and the old abandoned.

The conduct of the blind rat was really striking. In the first trial he did not notice the new path. In the second, however, he selected it after brief hesitation. The experience of the preceding night was thus strikingly effective. Singularly enough, he perfectly acquired the new association more quickly than the normals.[1] After the second trial he rarely went astray. In all three cases, however, the old habit was quickly broken, and a new, more advantageous one established. This preference for the shorter path is difficult to explain except upon the supposition that the path is known as shorter. To charge it up to the animal's "short-circuiting tendency," or his "tendency to eliminate useless movements" is to beg the question. Unless the advantage of the new path over the old is known in some way the old habit would persist simply in virtue of its own inertia. Again we cannot speak of a direct connection between *turning* in one direction and satisfaction; no such association is formed directly. A direct association is admitted; but it is formed only after experience with both paths, and deliberation often repeated between them. It is difficult to see what the association turns upon, if not upon distance-direction ideas of motor origin.

Why should not the repetition of the same motor expenditure establish in the psychophysical organism a path, the conscious concomitant of which is capable of being discriminated from similar feelings of other expenditures, and capable of reproduction under appropriate conditions?

The chief obstacle in the way of realizing to ourselves the existence and character of such representations as those suggested above is our lack of experience with pure tactual-motor ideas. It is a pertinent illustration of the influence of psychophysical limitation. We recognize that tactual-motor experience is fundamental in our own spatial perception; but it is so grown over and obscured by visual experience that it is next to impossible for us to realize in ourselves a pure tactual-motor image. No fusion of elements is more complete than this. The imperfect isolation we are able to give the tactual-motor element in

[1] A second normal was tried with results essentially the same as obtained with the other.

our own representations helps us, however, to imagine the pure idea of this kind. I observe that my own representation of the course through the maze is strongly motor. I find it just as impossible to see the course as a static visual image as to abstract the motor image from the interfused visual elements. I can alternately make one or the other focal, though the motor seems less *real* than the visual. This imperfect and mongrel tactual-motor experience of ours gives, however, a remote suggestion of the quality and feeling of such ideas in their pure condition. When we consider the clearness and definiteness of the spatial ideas of the blind deaf-mutes it ought not to be very difficult to conceive that an animal of poor visual endowment, or of semi-subterranean habits of life, may do a large part of his thinking in tactual-motor terms; and that the content may have a clearness and fullness of meaning hard for our visually over-slaughed minds to appreciate. With no feature of this study have I been so impressed as with the possibilities it reveals of thinking in motor or tactual-motor terms.

In carrying on this study I have profited greatly by aid and suggestions from many persons, and also from many books. I wish especially to express my indebtedness to Dr. E. C. Sanford for the initial suggestion, for ample laboratory facilities, and for continued interest and helpful criticism; to Dr. L. W. Kline for practical suggestions; to President Hall, and the members of his seminary, for salutary and stimulating criticism. The published works which I have found most helpful are those of Professors Lloyd Morgan, Wesley Mills, and Edward Thorndike, and the *Examen psychologique des animaux* of M. Pierre Hachet-Souplet.

# A COMPARISON OF JUDGMENTS FOR WEIGHTS LIFTED WITH THE HAND AND FOOT.

A. J. KINNAMAN, Fellow in Clark University.[1]

## INTRODUCTORY.

This series of experiments, begun in October, 1899, was undertaken for the purpose of determining the relative sensibility for "lifted-weights" through large and small muscles. At first attempts were made to use the large muscles of the leg and a flexor muscle of one of the fingers. That selection of muscles was soon abandoned because of the extreme difficulty, if not impossibility, of experimenting with the two muscles under anything like similar conditions. Finally it was decided to use the hand and foot as involving in a general way, large and small muscles. This then turns away, in part, from the original problem, but still involves the original to the extent that the muscles of the leg are larger than than those of the arm. Though suggestive, the experiments are in no wise conclusive as to the large and small muscle problem.[2]

After various attempts to eliminate the weight of the arm and leg, partly or wholly, this idea also was abandoned. It was taken up, however, in a second series of 4,500 tests not reported in this paper. The following is a report on 9,000 of the total 18,000 tests made. It is hoped that there is here furnished a reasonably safe answer to the question implied in the title of the paper, at least for one reagent.

The "standard weights" selected for this series of experiments are nine: 100, 400, 800, 1,200, 1,600, 2,000, 2,400, 2,800, and 3,200 grams respectively. Upon testing, the last weight proved to be about one-fourth of the reagent's maximum lift with the outstretched arm. The maximum lift for the leg proved to be about equal to that of the arm.

It was at first the intention to include larger standards up to

[1] This paper was prepared as a thesis for the A. M. degree at Indiana University. The experiments were made under the direction of Dr. W. L. Bryan. Valuable suggestions have been given, also, by Dr. J. A. Bergström, Dr. Sanford and Dr. Hall.

[2] Bernhardt experimented incidentally with large and small muscles in a study of "Muscular Sense." Archiv. f. Psychiatrie, III, cited by Ferrier's Functions of the Brain, p. 391.

6,400 grams, that is, to one-half of the maximum lift.[1] But the larger weights could not be experimented with extensively, without excessive fatigue or even physical injury. The likeness of this scheme to Fechner's makes comparisons of results practical and suggestive.

Jaccoud,[2] Leyden,[3] Bernhardt,[4] Sunkel,[5] and Hitzig[6] have made experiments on weights lifted by the foot. In Jaccoud's tests twenty-four ataxic patients lifted weights ranging from 100 to 3,000 grams to determine whether such patients could recognize weight, and to some extent the differences of weight which might be recognized by them.

Leyden, Bernhardt and Hitzig experimented with normal reagents by the method of "Just-observable Differences."

As yet no very extensive experiments have been made with foot-lifting, and none to compare accuracy of judgment through hand and foot in the same normal reagents, so far as I know.

## Apparatus.

The apparatus usually employed in lifted-weight experiments could not be used here. Fechner and Müller used essentially a rectangular base, from the corners of which brass rods extended upwards. These met in pairs at the ends, and were connected across by a wooden handle like that of a bucket bail. The weights were placed in the bottom of this rigid frame. While admirably suited for hand-lifts, this apparatus could not be used for the foot, but the lower portion of it could have been employed, doubtless, to some advantage. This is true especially of the method of arranging the weights and their respective instruments, *Zusatzgewichte*. In Fechner's apparatus this increment was placed in a small box which in turn was set into an opening in the center of the base. Thus every portion of his apparatus was firm and rigid.

Leyden in his foot-tests had a kind of pocket fastened to a band which hung over the instep. The weights were put into this pocket. Hitzig had the heel of a stocking sag downwards pocket-shaped and put the weights in this.[7] Bernhardt's reagent lay on his back and had a band around the instep of the foot. From the band a string passed under one pulley then upward over two others, and was attached to a small board on which the weights were placed.[8]

[1] Wreschner used 15 weights. The largest weighing 8,000 gr. Methodologische Beiträge. Leipzig, 1898.
[2] Paraplegies et l'ataxie du mouvement. Paris, 1864. p. 665.
[3] Ueber Muskelsinn und Ataxie. Virchow's Arch., 47, 321.
[4] Zur Lehre vom Muskelsinn. Arch. f. Psychia., III, 618.
[5] Inaugural Dissertation. Marburg, 1890.
[6] Neurol. Centralbl., VII, 249.
[7] Neurologisches Centralblatt. 7, 1888, 249.
[8] Arch. f. Psychiatrie, III, 628.

In attempts to devise apparatus equally suitable for both hand and foot, several forms were tried and rejected, which need not here be described. That used in these tests consisted of: (1) A board 6 x 8⅛ inches, the upper part of which was covered with a thick piece of cloth to avoid sounds and jars that might result from moving the increment; (2) Four strings attached to the four corners of the board, and meeting at a common point nine inches above the center of the board, where they were tied together and fastened to a single cord which continued upward about an inch. This single cord was inserted between the ends of the four strings and the collar above on the hand (or foot) so that if weight or increment should be placed, or slip, to one side, the variation would affect the direction of the suspended weight and the normal sensation as little as possible. To provide further against such effects the board rested on a thin cushion, so that the beginning and end of the lift might be affected as little as possible by any tipping of the board that might occur from a failure to center the weights properly; (3) A cylindrical band of heavy cloth with the diameter great enough to receive the fingers and palm of the hand (or the toes and a part of the foot). This band was sufficiently close-fitting to allow one to draw it to a given point and thus to keep it always in the same position and at a uniform distance from the joint. And since the arm is a lever this arrangement kept the distance from the weight to the fulcrum and power uniform. Over this band was fastened another of the same width but of greater length. It was sewed to the cylindrical band in a line along the upper side. The under side sagged downward about two inches, where it was gathered and tied to the single cord from below. This apparatus could be shifted easily from hand to foot, and was applied to about equal sense areas.

The apparatus, including the band, weighed 100 grams. The band alone weighed 26 grams. This series of experiments is at fault in that the weight of the band was on the hand constantly during each series of hand-tests. This was true, also, with the foot. Whatever absolute error in sensibility resulted from this source, its effect would be slight in comparing judgment for hand and foot, since both were subject to the same disturbing condition.

This error in apparatus should affect the absolute sensibility for the smaller weights more than for the larger ones, the band being proportionally a much larger part of the standard weight for small than for large weights. For 100 grams the weight of the band was one-fourth of the total standard; for 400, one-sixteenth; and for 3,200, only one one-hundred and twenty-

eighth. Leyden, Bernhardt and Hitzig appear not only to have left the apparatus on the foot while experimenting, but to have disregarded its weight, for they speak of distinguishing between 0 and 50, 83, 90 and 100 grams. In our tests the weight of the apparatus was always considered as a part of the weight to be lifted.

Standard gram-weights and books were used for standard weights. The increments were in the form of small muslin bags loaded with fine shot. In order to get results best suited for calculations by the tables in Dr. Sanford's Manual (p. 356), larger proportional increments were used for the smaller than for the larger weights, and for the foot than for the hand. These per cents were:

| Standards, | | 100 | 400 | 800 | 1200 | 1600 | 2000 | 2400 | 2800 | 3200 |
|---|---|---|---|---|---|---|---|---|---|---|
| Increments | Hand, | 10 | 7 | 5 | 5 | 4 | 4 | 4 | 4 | 4 |
| | Foot, | 20 | 15 | 8 | 7 | 5 | 4 | 4 | 4 | 4 |

The particular selections of the above per cents were based on recorded experiments and on some 5,000 preliminary experiments not reported here. The larger per cents were used with the smaller weights in an attempt to get something near eighty-five per cent. of correct judgments, as giving best results with the current tables. In some respects it would have been better to use the same per cent. of increment throughout, as Fechner did. Such a plan gives greater significance to the doubtful cases. The varying per cents of increments allow no common basis for calculation of such cases.

## Methods of Experimenting.

The experiments were made in sets of twenty of a kind divided about equally between heavy-first and light-first presentations, and were distributed as usual to avoid inequalities due to fatigue, training, the establishment of standard sensations, etc. Thus, a set of 20 tests with the hand, standard 100 grams, was followed by 20 tests with the foot, standard 100 grams; this, with hand, 400 and foot 400; and so on up the scale and back again. This was followed by foot, 100, hand, 100, etc., up and down the scale as before.

The individual tests were twenty seconds apart, and the two lifts constituting a test about two seconds apart. The latter time was not specifically measured, but was determined by the time required by the operator to place the increment on or take it off the apparatus, and to signal by tapping the hand or foot of the reagent. This time was very nearly uniform, and effort was made to keep it so. Variations from this source would be

almost equal for the foot and the hand. The average variation would render comparisons practically exact.[1]

Great pains were taken to keep the speed of the lifts uniform, and the same for both hand and foot. The speed has been shown to be a very important factor in the judgment of weights.[2]

An attendant noted the time for each test, moved the increment, signalled for the reagent by a gentle tapping of the hand or foot, determined whether a light or heavy presentation should be made, and recorded the results. The reagent had eyes closed and ears bandaged.

When tests of the hand were to be made the reagent sat on a dining-room chair by a table, with the arm outstretched and the hand projecting beyond the edge of the table. The board of the apparatus rested on the top of a piano stool, the supporting strings slightly loose. On receiving the signal the arm was kept straight and lifted. The stool was just high enough for the weight to clear it when the hand was about three inches below the level of the shoulder. The weight was then lifted about three inches and again lowered to the stool.

It is customary to gauge the height of a lift by a cord. The form of the apparatus made it quite impossible to do this. The writer is of the opinion that slight variations in height of lift are of but little consequence, since he appeared to base his judgments on changes of sensations at the beginning of the lift. After the lift the arm was again replaced on the table. As soon as the increment was changed and the signal given the lift was repeated. The result of judgment was announced by the reagent immediately after the second lift in each test. In the foot-tests similar precautions were observed, and the same plan of experimenting was followed.

In these experiments it will be noted that but two weights were used in testing a standard, 100, and the comparison weight, 110, when the smaller was presented first; or a standard, 110, and a comparison weight, 100, when the larger one was presented first. In the former case the increment was 10 per cent. of the standard; in the latter it was 9.09 per cent. This fact has been taken into account in working up the results and determining the curve.

[1] Edgar James Swift (*Amer. Jour. of Psychol.*, V, 1-19) has shown that reaction time is materially lengthened by distracting the attention with the ticking of a metronome. Accordingly one would expect the unavoidable distraction from a metronome to induce a higher per cent. of errors in weight-judgments. But Dr. Margaret K. Smith has noted in "Rythmus und Arbeit" that all but one of her reagents rendered better judgments when weights were lifted with the tick of the metronome than without it.

[2] Müller and Schumann: Pflüger's Arch., XLV, 37-112.

Some experimenters[1] use a standard weight and then comparison weights of a given per cent. larger and smaller than the standard. Thus with 100 standard would be used 110 and 90; one, ten per cent. greater, the other, ten per cent. less than the standard. If it is desired to get a judgment on the basis of a ten per cent. increment it appears that the method employed in our experiments is more nearly correct than the foregoing method. Taking 90 and 110 for comparison weights, the greater is more than 20 per cent. greater than the lesser, and the lesser slightly less than 20 per cent. smaller than the greater. In case of such a method being used, judgment is rendered more on difference between comparison-weights than between standard and comparison-weights. One could render judgment heavier than standard or lighter than standard, after experimenting for a while, quite as well without the intervention of a standard as with it, on account of the great difference in the comparison weights. I find that this view has been expressed lately by Martin and Müller in their experiments on lifted-weights. They say: "We are by no means of the opinion that a comparison of weights never takes place. Those comparisons, however, which are the easiest to determine, are, strange to say, not comparisons between standard and its variables, but a comparison between a variable which has just been hefted and the variable of the immediately preceding experiment."[2]

In these experiments the reagent knew: (1) That there would be twenty tests in the set, divided as stated above; (2) That not more than four of a kind would come together; (3) Sometimes he knew that he had missed a given number in a previous set of the kind about to be undertaken; (4) That about such and such results might be expected from the hand-tests, *e. g.*, the light-first tests would show a higher degree of sensibility than the heavy-first tests would.

This makes the series, so far as absolute results are concerned, subject to some of the adverse criticisms directed against Fechner's work,[3] in addition to those already mentioned. Fechner, it will be remembered, was both operator and reagent in his experiments. Introspective observations, however, seem to indicate that the knowledge named above acted more as a spur to attention than to any other end.[4]

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[1] Wreschner: Metholodische Beiträge, Leipzig, 1898. Martin and Müller: Zur Analyse der Unterschiedsempfindlichkeit, Leipzig, 1899.

[2] *Amer. Jour. of Psychol.*, XI, 269.

[3] Fullerton and Cattell: "On the Perception of Small Differences," p. 116.

[4] For experiments with similar limitations, and a discussion of the same, see Psy. Rev. Sup. I, 31.

Practically two forms of answers were given; "second lighter, or heavier," when the distinction was clear; "second lighter, or heavier, D [doubtful]" when there was a very low degree of assurance. Introspective observations of various kinds were often noted in addition to the foregoing answers.

The results were worked up according to the method of Right and Wrong Cases, following Fullerton and Cattell's suggestions, and using the tables and methods of calculations offered by Dr. Sanford in his Manual.

In these tests an answer was always required from the reagent, however great his feeling of doubt. Investigators have not always dealt with the doubtful cases in this way. Fechner divided the doubtful cases equally between right and wrong judgments.[1] Wreschner threw them away;[2] Kraepelin, Jastrow, Fullerton and Cattell, and Higier required that the reagents should always answer no matter how great the feeling of doubt.

## Table and Curve.

This curve and the accompanying table show the sensibility of the reagent's hand and foot and of Fechner's hand according to the method of Right and Wrong Cases. In determining sensibility from Fechner's experiments his right-hand tests were used. That the two series of experiments might be put on a par as far as possible, the three, five and ten hundred-gram tests of his were selected from sets where an increment of eight per cent. was used, while the others were taken from his tests with a four per cent. increment. Thus in this and in his series we have a large per cent. of increment with the lighter standards and a small per cent. with the heavier standards.

A comparison of Fechner's sensibility-curve with that of the reagent's hand in these tests will show them to be very similar in general form, though slightly different in detail and elevation. Individual differences, differences in apparatus, differences in methods of experimenting, and in the number of standards taken will, doubtless, account for the slight differences in results, was determined from the data on page 186, Vol. I, Psychophysik, 1889, with a full realization that he and the writer dealt quite differently with their doubtful cases. The comparison, therefore, is not fully justifiable, but is offered for whatever it is worth.

In Fechner's experiments six standards were used, while nine were used in these. This would tend to make his curve appear the more regular of the two.

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[1] El. d. Psychophysik, VIII, 72.

[2] *Amer. Jour. of Psychol.*, IX, 592. Gesellschaft fur psychologische Forschung. Heft II (III Sammlung).

*Curves of Sensibility or Power of Discrimination.* (*9,000 tests.*)

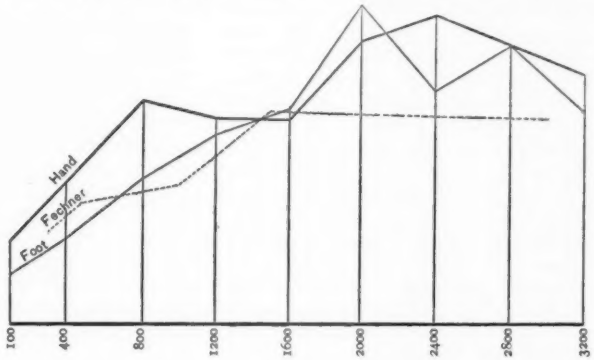



*Table of Sensibility.*

| Standard W'ghts. | Hand. | Foot. | Standard Weights. | Fechner. |
|---|---|---|---|---|
| 100 | .093 | .053 | | |
| 400 | .156 | .094 | 300 | .103 |
| 800 | .242 | .160 | 500 | .132 |
| 1200 | .228 | .207 | 1000 | .151 |
| 1600 | .226 | .231 | 1500 | .232 |
| 2000 | .328 | .347 | 2000 | .232 |
| 2400 | .336 | .251 | | |
| 2800 | .308 | .308 | | |
| 3200 | .276 | .235 | 3000 | .229 |

Individual differences are often very great, however, and they alone could account for the variations in the curves.[1] For example, by the method of just observable differences Leyden's reagents showed a fineness of judgment for the foot double that of Bernhardt's. Müller and Schumann's subjects were less than half as sensitive as Fechner for certain weights.[2]

The grasp or hand-hold is probably an important factor. Cattell and Fullerton (Perception of small differences, page 118),

[1] Griffing: Psy. Rev. Sup., I, 42.
[2] Schäfer's Text-Book of Physiology, II, 1021.

with small wooden boxes to be grasped by the sensitive fingers, obtained an average sensibility of 161 when a standard weight of 109 grams was used. In these experiments, however, with a less active grasp and an appeal to a less sensitive dermal area[1] the sensibility was but .093, or about 9-16 as fine as theirs. Active effort undoubtedly enables one to judge more accurately than passive effort. Merkel found judgment based on active movement of the beam of a balance to be to passive pressure as 17 to 13.[2]

For the lighter standards this apparatus appealed to very small areas; but for the heavier standards, to large areas. The lightest weights merely produced slight movements in the band when the weight was lifted. The sensations from lifting the two weights appeared to differ only in that the band with the heavier weight rubbed the skin a little more than the lighter, usually along the side of the index finger. Fechner's apparatus, for the lighter standards, appealed to a larger area. Whether this difference of area affects the sensibility is a matter of dispute. Lemon[3] says it does, while Griffing[4] says it does not. In any event the curves show that where the dermal areas appeared to vary most, there the sensibilities vary most; yet the variation of sensibility may be due to other causes than differences of area.

There is probably a significant difference in the rigidity or firmness of the apparatus used. That used by Fechner, Müller and others consists essentially of a firm metallic or wooden frame, while that used here consisted largely of fabrics. With the solid frame the weight clears its base of support more suddenly than with our apparatus. With a rigid piece of apparatus a given speed of lift will make the changing sensations at the beginning of the lift pass more quickly than where the apparatus is less rigid. With our apparatus there is but little tendency for the weight to *fly up*. It is probable, also, that slight variations in the speed of the lift will affect judgment less with this apparatus. Upon the whole the other apparatus appears to be more exact, and should be used wherever possible.

Fechner's and these curves follow each other much more closely than any others[5] that the writer has seen.

As for the hand and foot curves in this chart, they closely resemble each other, even in detail, except at 2,400 grams. That variation is probably accidental.

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[1] For points, the sensibility of the tip of the finger is to that of the palm of the hand as to 1 and 5. Weber; cited by Bain: Senses and Intellect, 190.

[2] Phil. Stud., V, 287.

[3] The Skin considered as an Organ of Sense, p. 46.

[4] Psy. Rev., 1895, p. 128; also Supplement I Psy. Rev., p. 47.

[5] Hering, Müller and Schumann.

The curves show at first a rapid increase of sensibility to about the 800-gram standard, followed by a less marked increase up to 1,600. Then they rise rapidly to a maximum sensibility at 2,000 and 2,400, after which a slower decrease sets in. The cause of this change at from 800 to 1,200 grams is discussed under Hypothetical Considerations, further on in the paper.

The few tests made with much larger weights seem to indicate the continuation of this downward tendency in the curve, showing a further decrease in the sensibility. These tests with very heavy weights were not numerous enough to demonstrate anything, but at 6,400 grams, one-half of the reagent's maximum lift, they indicated a sensibility of not more than .125, which is less than that obtained with a standard of 800 grams. In the latter part of the paper, under "Hypothetical Considerations," is offered an explanation for this increase of sensibility, observed by all experimenters, as the scale of weights is ascended. In that section of the paper an attempt will be made to show that it is due to the influx of additional bases of judgment. The decrease of sensibility in the very large weights will be accounted for on the basis of an influx of distracting influences.

The difference in sensibility of the hand and foot, beyond 1,200 grams, is very small. The larger difference with the lighter standards may be due to finer dermal discrimination in the hand than in the foot, the basis of judgment for these standards being largely dermal.

Dr. Krohn has shown[1] that exposed surfaces locate better than portions usually covered with clothing, and Weber[2] found the sensibility for points on the dorsum of the foot to be to that of the palm of the hand as 5 to 18. Todd and Bowman[3] hold that the relative power of different parts to estimate weights corresponds very nearly with their relative capacities of touch, and Bloch[4] shows the weights, for just observable pressure for the palm of the hand and dorsum of the foot, to be as 1 to a weight ranging from 40 to 60.

These views put together make it appear altogether probable that there is a decided difference in the pressure and touch sensibility, for the dorsum of the foot and for the palm of the hand, and that this may account for the difference of sensibility as shown in the first part of the curves. But this difference is so small that it may be said, in general, that within the first quarter of the reagent's range of lifts, the sensibility of the foot for

[1] Psychol. Rev., 1894, p. 326.
[2] De pulsa, resorptione, auditu et tactu.
[3] Brit. Med. Jour., XIII, 430.
[4] Arch. de physiol., III.

lifted-weights is about equal to that of the hand, even though the foot had never before been used in lifting weights.

TABLE II.

*Percentage of errors in the successive tests in all of the sets of twenty for both hand and foot.*

| No. of Test. | % of Wrong Judgments. | No. of Test. | % of Wrong Judgments. |
|---|---|---|---|
| 1 | 30.23 | 11 | 22.66 |
| 2 | 18.22 | 12 | 24.88 |
| 3 | 21.33 | 13 | 22.66 |
| 4 | 19.77 | 14 | 18.88 |
| 5 | 23.77 | 15 | 23.99 |
| 6 | 24.00 | 16 | 19.77 |
| 7 | 23.50 | 17 | 23.12 |
| 8 | 21.11 | 18 | 23.55 |
| 9 | 20.88 | 19 | 26.21 |
| 10 | 26.21 | 20 | 24.33 |

This Table shows that on the average the first judgment in each series was poor, there being an average of 30.23 per cent. of errors for both hand and foot. While the average of the second tests shows only 18.22, or the smallest average per cent. of error in the whole set of twenty tests. This sudden improvement, after the first test, is the most characteristic thing shown in the Table. It is probably due to accommodation and setting up a standard sensation. It could hardly be due to increase of effort at attention over that rendered to the first test. It seemed to have taken the time up to the second test for the reagent to adjust himself to the series. Usually the attention was most intense at the beginning of the series, then a restful feeling of assurance, after three or four tests, was very common. If it be true that we underestimate weights when attention is intense,[1] that should not affect our judgments in comparing weights, as both standard and comparison-weight would appear equally reduced.

When concentration failed, as it did of necessity, occasionally, it never occurred with first, second, third, fourth or fifth tests. Introspectively, at least, the reagent would be of the opinion that attention was always most intense with the first test. Whenever attention was grossly at fault the reagent, without knowing the correctness or incorrectness of his answers, requested to be taken again over the last four or five tests, he not knowing exactly how much territory he was retracing. But every effort was made to keep attention uniform.

These tests it will be remembered were made twenty seconds apart. From last lift of one test to first of the next was about sixteen seconds. The standard sensation bridged over this time so distinctly that the reagent was able to render, in mind, after

[1] Psychol. Rev., I, 44.

the first lift of each, nearly one-half of the time, the same decision that he did render after making both lifts. The standard or absolute sensation, therefore, appears to have been an important factor in forming the judgments. In this view the charts and introspections here quite agree with Angell and Harwood, and with Martin and Müller, as stated by Angell in his review of the Martin and Müller article.[1]

Following the sudden improvement after the first test there is a slight general increase of error toward the close of the series, probably due to fatigue and diversions of attention. The principal factors entering into these variations are attention, fatigue and the establishment of standard sensations.

It is evident that the disturbances on the whole are not very great. For example, the first ten tests show 22.90 per cent. of errors, and the last ten 22.91 per cent.

But it is impossible in this study to isolate and measure these factors. Fatigue and inattention would tend to increase the wrong judgments, while the establishment of a standard sensation would tend to a decrease of the same.

The effect of training or practice was calculated and found to be very slight, the judgments through the foot showing a little more improvement than those through the hand. This extremely slight improvement, as shown in these experiments, is not so remarkable when we remember that about 5,000 tests had been made before this series was taken up.[2]

## Summary of Results.

I. In answer to the main question of Comparison of Judgments: (a) The weights were discriminated a little better through the hand than through the foot; (b) The relative difference was greater for the small than for the large standards; (c) Both showed a lower degree of sensibility both above and below 2,000-2,400 grams, or one-third of the reagent's maximum lift; (d) The practice effects were small and about the same for both hand and foot; (e) The discrimination of the heavy-first presentations differed from the light-first presentations in the foot-tests less than in the hand-tests. (This fact has not been discussed in this paper. For the hand the variation was slightly above the average. For the foot it was probably less than the average hand-variations.) (f) The sensation seems to be partly a touch and partly a muscular sensation as observed by Fullerton and Cattell; (g) The fineness of discrimination is not materially disturbed by the fatigue resulting from

[1] *Amer. Jour. of Psychol.*, 1899-1900, p. 260.

[2] On effect of training see Wreschner, Metholodische Beitrage, p. 183. Leipzig, 1898, pp. 238.

a short series of tests; (h) Standard sensations play an important rôle in a series of like judgments; (i) The second test of the series are judged better than any others.

II. Some Introspective Observations. (a) The judgments in tests on 100-gram weights seemed to be based quite exclusively on mere touch-sensations; (b) The judgment seemed to be based more on the changes in stimuli that occurred at the beginning of the lift, than on any constant sensations after the weight once cleared its base of support. The best results were obtained by directing the attention to the first sensations. Swinging the weight up and down did not seem to improve the discrimination much, if any; (c) The accuracy of judgment varied greatly, sometimes even with no apparent cause; (d) Very quick and very slow lifts were hard to judge; (e) In lifting the large weights the judgment seemed to be based principally on "muscular sensations;"[1] (f) Very small increments make judging very fatiguing by their great demands on attention; (g) One can distinguish quite noticeably when lifting very large weights, that he is beset with a focal[2] sensation, and with numerous marginal sensations, some having only a distracting influence upon the attention, such as noises, heat, the pains from uncomfortable positions, and muscular pains; and others, that must be regarded as auxiliary sensations to the focal sensation. They, with the focal sensation, figuratively, in a heap, constitute the real basis of discrimination. Among these marginal auxiliary sensations the following were pronounced: (1) Touch sensations around the grasp;(2) Pressure sensations;(3) Changes in the pressure of the back against the back of the chair; (4) Increased pressure on the seat of the chair; (5) Feet pressing more firmly on the floor; (6) Other hand putting forth effort wherever it may be placed; (7) Interference with breathing; (8) Interference with circulation; (9) Intercostal and abdominal muscular sensation.

Some of these may pass over and become sources of distraction rather than auxiliary sensations.

## Hypothetical Considerations.

The following hypotheses are based partly on the data determining the curve given in the first part of the paper, and partly on the introspections noted as the experiments were being made.

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[1] Sensations based on specific sense-organs in muscles, tendons, joints, and all of the accessory organs of movement. Schäfer's Text-Book of Physiology, Vol. II, 1006. Macmillan, New York, 1900.

[2] By *focal sensation*, it is intended to designate one toward which the mind is primarily directed.

*Basis of Judgment.*

In stating the basis of judgment for lifted-weights it is convenient to divide the field of sensations into two classes, focal and marginal. When weights are being lifted the focal sensation is usually, though by no means always, the sensation for which the stimuli change most vividly.

Besides this focal sensation there are many marginal sensations in consciousness down to a feather-edge in "subconsciousness." Some of these tend to draw attention away from the focus, and thus to induce poorer judgment: others seem to go along with the focal, to be auxiliary sensations, and by their presence to induce a finer discrimination.

These marginal auxiliary sensations are often very marked where hardly anticipated. When conducting some tests where the reagent lay on his side on the floor, several judgments with the larger standards were rendered with the attention fixed upon the tendency to slide along the floor in the opposite direction from the pull that was being made. Surprisingly enough the feeling of assurance and the correctness of the decisions were quite as good as when the attention was directed to the muscular sensations of the arm. But the fact of these marginal sensations does not prove that any of them are auxiliary in forming the judgment.[1] Jastrow and Bruce bandaged the arm tightly so as to empty most of the blood, and then rubbed the skin with ice. This greatly reduced the sensibility of the skin, but did not affect the judgment for lifted-weights. (Letter to the writer.) Here subtracting some of the marginal sensations did not affect the judgment.

"According to the law of perception[2] of weight by the sense of cutaneous pressure alone, it requires the addition of one-third of the original weight, whatever it may be, to produce a distinctly perceptible difference; but in Bernhardt's experiments on the foot it was found that the addition of from three to five Loth (1½ to 2½ oz.) to an original weight of from a pound to a pound and a half could be distinctly perceived, which is less than one-half of the increment perceptible by cutaneous pressure alone.

In regard to the discrimination of weight by the finger, the sensibility was found to be much finer. Three drachms could be distinctly differentiated from nothing, and to heavy weights (say 1 pound) the addition of five drachms was dis-

[1] Pierce and Jastrow (Memoirs of the Nat. Acad. of Sci., III, 1884,) showed that even the mere guesses on small differences fall according to law predominantly on the side of right judgments. Thus it appears that the subconscious states are factors that must be taken into account in judgments.

[2] Ferrier: Functions of the Brain, p. 391.

tinctly perceived, *i. e.*, a difference of about 1/16, a power of discrimination which corresponds pretty nearly with that of the muscular sense, which is capable of detecting an addition of 1/17 of the original weight. These results, therefore, indicate that the discrimination was much finer than could be effected by the sense of pressure alone, and that, therefore, it depended on muscular discrimination."

It seems that the presence of marginal sensations on which, singly, a fair judgment could be based, and all of which are induced by stimuli from a single source, within normal limits, should increase the accuracy of judgment. The presence of such a sensation may decrease the nicety of judgment of the focal sensation. But still it would have to be shown whether the decrease in the focal sensation is not more than compensated by such a marginal sensation as described above.

In whatever position one may be when he makes a lift the auxiliary marginal sensations are present. They vary greatly as larger standards are used. With their cumulative influx, up to certain limits, they probably increase the acuteness of discrimination. When very great, however, they may become sources of distraction.

With the very small weights the marginal consciousness is very limited in extent. Until a given degree of motion or tendency toward it is reached at any sensitive part of the body, no sensations result. Some organs and processes are disturbed earlier than others as the scale of weights is ascended. Hence, this gradual increase in the extent and intensity of the marginal consciousness. So it happens that, as the scale of standards is ascended, new marginal sensations keep coming in and rising higher and higher in consciousness. Some of the marginal sensations may become so strong as to take over the focus to themselves. And this is just what appears to happen, the muscular sensations which are at first marginal, later, become focal, and with the very heavy weights, sources of distraction, while the dermal sensations become marginal. Biedermann and Löwit found that the dermal sensibility to pressure decreases very rapidly above 450 grams. At 450 the least observable difference was 1/69, and at 500 grams it was 1/20 of the standard weight.[1]

This influx of marginal sensations, and transposition of focal sensations, from the tables and from the introspective observations, seems to have been most marked at from 800 to 1,200 grams. At 2,800 and 3,200 breathing and circulation were quite perceptibly disturbed. With the very large weights the

[1] Hering: Sitzungsber d. Wiener, Akad., LXXII, 342. Ladd's Physiological Psychology, 368.

sensations of strain about the shoulder, chest and abdomen may become focal.[1] With the latter weights also came certain marked distractions, as, muscular-pain from the heavy lift, and an unavoidable trembling of the arm.

It is probably entirely impossible to isolate completely these various sources and bases of judgment so as to determine the comparative worth of each for various standards, and within what limits Weber's, Fechner's or any other law holds. It may be that some law holds for each of the several bases after it becomes a factor in the discrimination, probably Weber's, but that no law has been or, possibly, can be formulated to apply to the sum of them.

Views of experiments are not wanting on the subject. G. E. Müller has called attention to the probability of a gradual influx of additional sensations.

With the larger weights come first muscular sensations with the upper and fore arm; and with very large weights, from the muscles of the chest and shoulder. One must ask, then, to what extent Weber's law is valid. The lifted arm may be a part of the lifted weight. To determine the validity of the law one must know whether any, or part, or all of the arm goes in with the given weight to make up the weight on which we really base our judgments. Moreover, the antagonizing force of the opposite and adjacent muscles cannot be known.[2]

Hering, long ago in a letter to Fechner, pointed out the probability of a complex of sensations as the basis of judgment.

"At the same time," says Bastian,[3] "a number of impressions emanate from the back of the hand, which, so far from aiding him in his judgment, would be a source of mental distraction. Then I would say, also, that, in estimating such weights as Weber employed, the person would almost certainly have experienced sensations emanating from the muscles themselves; and there is really no reason for believing that these sensations, derivable from a muscle which is contracting somewhat strongly, do not reveal themselves to us by means of ordinary sensory nerves in the muscles. I believe that they are due to the common sensibility of the muscle testifying to its own organic conditions, and that these sensations may well be part components of the impressions by which we are enabled to judge of differences of weight. And if this be the case, then it was to be expected, as I think, that there should have been a much greater power of discriminating differences of weight when the common sensibility of muscle was allowed to act in conjunction with the tactile impressions, than when these latter

[1] Ferrier's Function of the Brain, p. 392.
[2] G. E. Müller: Grundlegung der Psychophysik, p. 202.
[3] Brit. Med. Jour., XIII, 1869, 439.

were appreciated alone and apart from all distracting influences." He holds the muscular sense, however, to be only a fraction as delicate as the pressure sense.

We note that our view does not agree with Ferrier's[1] which seems to set aside all joint, tendon and purely muscular sensations in the arm. He says, "I am of the opinion that the discrimination of heavy weights calls into play the general sense of effort, which, as we have seen, is the more properly ascribed to the region of the respiratory muscles, and that the discrimination in this case is affected by the amount of bodily strain and fixation of the muscles of the chest necessary to support a heavy weight; and that it is not a question of the muscular sense of the limb at all, unless general strain is absolutely eliminated by continuous and easy respiration during the trial. When this is eliminated it will be found that the sense of local resistance is the only element in the discrimination of weight."

Weber considered that the reason for finer judgment in lifting, over that of mere pressure, was due to the muscular sense coming in as a further basis of judgment.[2]

The above views, except Ferrier's, seem to tally with the writer's introspections which may be restated briefly thus: Judgment of weights is based on focal and auxiliary marginal sensations, which change greatly as the scale of weights is ascended. The focal sensation changes from dermal to muscular, but may be voluntarily placed elsewhere; the marginal group constantly increases; and some sensations pass from auxiliary to distracting sensations.

In lifting a weight there is probably a normal rate of change in sensations at which the total amount of change can be judged most accurately, varying, to be sure, with different reagents. Change may be either too rapid or too slow for accurate judgment. J. S. Lemon[3] states that great pressure can be made without producing consciousness of the fact, providing that it be applied very gradually. Fontana, showed that a slight pressure on a nerve might be increased gradually until the nerve was crushed, without producing motion in the corresponding muscle. Heinzmann[4] and Fratscher[5] boiled reflex frogs without sensation enough to produce motion by applying the heat very gradually.

Very rapid changes make strong sensations, but sensations lacking nicety for distinction. Fullerton and Cattell,[6] on the

[1] Functions of the Brain, 392.
[2] Living Age, XIII, 12.
[3] Skin Considered as an Organ of Sensation: Whiting, and Whittaker, Gardner, Mass., 1899.
[4] Archiv. f. die gessammte Physiol., VI, 1872, 222.
[5] Jenaische Zeitschrift, I, 1875, 130.
[6] Perception of Small Differences, p. 131.

basis of a few experiments, held that the rate of lift does not increase the probable error. In changes of pressure it is claimed that the more rapid the changes the greater the accompanying distractions.[1]

It is precisely this change of sensations, occurring from the beginning of the lift until the weight clears its support, that we compare and judge, rather than the totality of sensation after the weight is up. The reagent will judge best when the speed and other conditions are most favorable for his taking these changes into full consciousness. If the normal speed is maintained, the height of the lift is of but little consequence, providing the reagent holds firmly to the changes which occurred at the beginning of the lift. But when the reagent defers judgment, he is liable to question his impressions and do little more than guess. Even when judgment is not at once declared and dismissed, one is apt to conclude that his impressions may have been in error. Moreover, the pressure, and probably other sensations, decrease rapidly after the application of the weight.[2] From our standpoint the basis of judgment appears to be the memory of a former change of sensation as compared with a present changing sensation.

The notion that the judgment is based partly on the sense of effort we pass by. The sense-of-effort view has been pretty generally abandoned.[3] If sense of effort exists at all it may be said to assume the relation of marginal sensation. Neither does it appear, with this apparatus, which picks up the weight more gradually than does Fechner's and Müller's apparatus, that the speed of the lift is so important as Müller and Schumann[4] found it to be. The speed, here, appears to be but one of the marginal sensations.

### *Interferences of Sensations.*

Judgments of sensations are disturbed by two kinds of sensation-interference: (1) distraction, and (2) fusion. Distraction through sensations occurs whenever sensations arise which tend to draw the focus of attention to themselves, and thus away from any sensation or group of sensations which one is trying to judge. Interferences of this kind have been referred to at various places in this paper, and, for our present purposes, do not need to be discussed further here. This kind of interference has been pretty generally recognized, and has been studied rather extensively. Distractions arising

[1] Hall and Motora: *Amer. Jour. of Psychol.*, I, 87.
[2] Psychol. Rev., Sup. I, p. 30.
[3] Schäfer's Text-Book of Physiology, II, p. 1002.
[4] Pflüger's Archiv., XLV, 36-112.

from the fusion of sensations seem to have been studied but very little. I believe it would be entirely practicable to study them experimentally. The following is offered as an hypothetical application of this variety of interference involved in lifted-weights.

Sensations very unlike and with their stimuli remotely located are readily distinguished as different. But as the stimuli approach each other in kind and location, it becomes more difficult to discriminate them. Again, the more usual and neutral as to pain and pleasure a sensation is, the more readily and completely can it be disregarded for others to which we wish to give especial attention.

Now, it was quite clear to those who tried the apparatus used in the foregoing experiments, that the conscious basis of judgment was tactile for the 100-gram standard. The marginal auxiliary sensations, if present, were wholly subconscious. This tactile sensation is altogether unlike the sensations arising from lifting the arm, and the latter, if not continued too long, are usual, normal, neutral. The mind should, therefore, readily differentiate these two sensations and neglect the usual neutral arm-weight sensations. It would seem, then, that the weight of the arm does not in the least interfere with the judgment of small weights, with this apparatus, any more than the breathing, touch of the clothing, or any other neutral sensation interferes with thinking the binomial formula in algebra. In our opinion, as the standard increases in weight, the marginal sensations loom up in consciousness, more and more, and gradually the muscular sensations gain the ascendancy. Then some of the sensations arising from lifting the weight become identical in kind and location with those arising from lifting the arm and the mind cannot, naturally enough, distinguish the two. What was a four per cent. increment on the weight is now less than four per cent. on the total amount of standard-weight and some arm-weight measured in terms of sensation. This reduction of the per cent. of increment in the focal stimulus tends to decrease accuracy of judgment for a given standard and a given per cent. of increment of it. The weight of the arm thus gradually insinuates itself into the standard.

It is not probable that we judge one weight as just a little more than another; that would contradict all of our experience with Weber's law. If that were true the same difference would serve for distinguishing any two weights, no matter how large or small. The increments must be regarded as per cents of the standards, and since the weight of the arm seems to change this per cent. as the scale of weights is ascended, the weight of the arm may be expected to interfere with the judgment.

But if the weight of the arm does not get confused with the standard-weight, or rather if the sensations do not fuse for the small weights and this does happen with the larger ones, then why is the discrimination not poorer for the larger than for the smaller weights? If our hypothesis shears close enough to the truth, then it would follow that the marginal subconscious sensations more than compensate the loss through the interference of arm-weight.

This, it seems, may explain, in part at least, how the weight of the arm interferes in the discrimination of lifted-weights. The hypothesis is offered as such only, and not as something proven, simply a possible explanation for a part of the problem.

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# PSYCHOLOGICAL LITERATURE.

*Examen psychologique des animaux. Nouvelle méthode expérimentale de classification des espèces au point de vue psychologique.* Par PIERRE HACHET-SOUPLET. Paris, Schleicher Frères, 1900. pp. xvi-162.

The author of this interesting book points out that comparative psychologists have missed the most natural and fruitful method of studying animal minds, in that they have failed to appropriate and use systematically for scientific ends, the procedure employed by professional trainers for practical ends. The book in question presents the results of a limited application of this method; but for completeness and certainty, such work needs to be done in connection with great zoölogical gardens. M. Hachet-Souplet announces an understanding with the authorities of the *Jardin d'acclimation*, which makes the realization of his project of a great zoölogical laboratory, where such systematic study may be carried on unlimitedly, only a matter of time. Certainly much is to be hoped for from the adoption and application by psychologists of the methods of testing animal intelligence in this large and free, and at the same time, strictly experimental way. Suggestions of a like tenor, but much less comprehensive, have been made by Professor Lloyd Morgan; of which, however, M. Hachet-Souplet appears to be ignorant.

Inasmuch as the minds of animals vary independently of morphological characters, the author demands a new classification of species based strictly upon psychological characters. This demands study of animal minds on their own account, absolutely free from morphological leading strings. It will be time enough to reconcile the morphological and the psychological classifications after each separately has been made out with some degree of completeness. The psychological criteria are: what does the animal do under given conditions, and what influences does he respond to. Upon the basis of his somewhat wide and varied employment of the *méthode de dressage*, the author proposes a tentative scheme of classification embracing types of most of the morphological categories. This scheme consists of three categories: animals whose sole psychic attribute is excitability; those subject to coercion; those subject to persuasion. The first category is self-explanatory, and includes only the protozoans. The second category contains those animals possessing primitive instincts—nutrition, reproduction, and conservation (*défence*)—and functional memory, *i. e.*, memory connected always with some bodily function. The animal perceives, in the external world, the conditions of satisfying the primary instincts; but this consciousness is strictly connected with the return of functional need, and leaves no independent image in the nervous organism. There are two sub-categories based upon the relative complexity of the functional memory. This category includes animals as widely variant as Medusa and deer. Training is by *coexcitation*, *i. e.*, by modifications of external conditions or fear—whereby the *sûrs instincts* are readapted.

The third category contains all the higher animals. They possess intelligence: *i. e.*, their brains are capable of preserving impressions of perceptions independent of immediate functional needs. Such an

animal is like a closed vase in which ideas develop, jostle each other, and combine. The result of the jostlings and combinations is a *détermination*, in the sense of the domination in consciousness of one or other of the ideas. In animals, as in man, this gives the illusion of choice and free intelligence. Animals in this category are capable of being persuaded, *i. e.*, of conceiving cause and effect as man conceives them. They understand when man seeks to show them by "*une mimique expressive*," the relation of cause and effect. Such mentation M. Hachet-Souplet thinks he has demonstrated in some of the higher animals. There are three sub-categories, dependent upon the degree to which the animals are subject to persuasion; and upon the presence of secondary (acquired) instincts. He is a warm partisan of the "lapsed intelligence" theory, and thinks the thesis easily demonstrable by his method of investigation. This category includes types as remote as the ant and the chimpanzee. To the higher animals are attributed imagination, abstraction, æsthetic sense, personality and other high mental powers. These attributions, however, are made guardedly and on the basis of experiment; not at all in the loose manner of Romanes, whom M. Hachet-Souplet constantly criticises.

The obvious strictures upon the book are: the somewhat superficial character of some of the psychological analysis; the ignorance or disregard for some important recent work in animal psychology—there is no mention of Morgan, Thorndike, Bethe, Wasman, Peckham *et al.*; and the fact that his zeal for the evolutionary philosophy (Spencer's) occasionally carries him, in his interpretations, beyond his facts.

Nevertheless, this book is a valuable and interesting contribution to positive comparative psychology—and, withal, brilliantly written.

W. S. SMALL.

*Psychology, Empirical and Rational.* (Stonyhurst Philosophical Series, No. 5.) By MICHAEL MAHER, S. J. Fourth edn., revised and enlarged. London, New York and Bombay. Longmans, Green & Co., 1900. pp. xxii, 602, xii. Price, 6/6.

In this new edition, the modifications up to ch. ix are slight, except that more space is devoted to physiology and psychophysics. Chs. xiv (origin of intellectual ideas), xvi to xix (attention and apperception; development of intellectual cognition; rational appetency; free will and determinism), xxii (false theories of the ego) and xxiv (immortality of the soul) are almost wholly new; the supplement on hypnotism, and the criticisms of the theories of James and Höffding, appear for the first time. The historical sketches have throughout been substantially increased.

For the rest, the plan and scope of the work remain as before. Thomas Aquinas is the constant standard of reference; and, while the author does good service in calling the attention of scientific psychologists to authors with whom they are but little, if at all familiar, he has himself no adequate knowledge or appreciation of the course of scientific psychology.

E. B. T.

*From India to the Planet Mars: a Study of a Case of Somnambulism, with Glossolalia.* By TH. FLOURNOY, Professor of Psychology at the University of Geneva. Translated by D. B. Vermilye, New York and London; Harper Bros., 1900. pp. xx, 447. Price, $1.50.

The original of this translation was reviewed in the *American Journal of Psychology* for April, 1900 (Vol. XI, pp. 428 ff.). As, however, the book is attracting much attention, both from psychologists and from the general public, and as its contents will strike different readers very differently, I take the opportunity afforded by the appearance of

an English edition to say a few words about the remarkable case of automatism described in its pages.

The subject of the memoir is, as is well known, a certain 'Mdlle. Hélène Smith,' an unmarried woman of about thirty-five years of age, "earning an honorable living as an employee of a commercial house." M. Flournoy has been at great pains to gather data concerning 'Mdlle. Smith's' childhood and youth—wisely, since such data are of extreme importance for the understanding of her story. We learn that she was a child of somewhat dull intelligence, at least as measured by school standards (p. 18); that she was given to day-dreaming and reverie; that she was subject to occasional hallucinations; that she felt herself out of place in and superior to her family surroundings, so that she "one day seriously asked her parents if she was their daughter" (27), and her father and brothers were apt to rebuke her for pride; and that she was of a melancholy, timid, nervous disposition. "Hélène Smith was certainly predisposed, both by heredity [on the mother's side] and temperament to become a medium" (19). Nevertheless, she 'outgrew' these systoms of nervous disorder: the "teudency to mental disintegration, which marked the years of puberty, was succeeded by a progressive diminution of these troubles." "We may infer . . that the ebullitions of the imaginative subconscious life gradually became calm after the explosion of the period we have mentioned. . . A certain equilibrium was established between the necessities of practical life and her inward aspirations." "We may presume that this harmonization . . . . would in time have perfected itself, and that the whole personality of Mdlle. Smith would have continued to consolidate itself" (32 f.). In a word, if she had been left to herself, 'Mdlle. Smith' might have become a normal personality, and have been the healthy mother of commonplace children (32, 131). Fortunately or unfortunately, as the reader may choose, she fell (at about the age of twenty-seven) under the influence of some spiritualistic friends; "spiritism . . . . came all of a sudden to rekindle the fire which still slumbered under the ashes, and to give a new start to the subliminal mechanism which was beginning to grow rusty." This fact—the fact of the spiritistic manufacture of 'Mdlle. Smith's' mediumship—should not be overlooked.

Another point of interest is the following. It was "the age of puberty" in 'Mdlle. Smith' that "began to favor the development of the Oriental visions." Now the 'Leopold' of the revived subliminal imagination has his origin, in M. Flournoy's judgment, in the sexual coenæthesis of the medium. "He represents . . . . the synthesis, the quintessence,—and the expansion, too,—of the most hidden springs of the psychological organism. He gushes forth from that deep and mysterious sphere into which the deepest roots of our individual existence are plunged, which bind us to the species itself, . . and where confusedly spring our instincts of physical and moral self-preservation, our sexual feelings" (87). "The real and primordial origin of Leopold is to be found in that deep and delicate sphere in which we so often encounter the roots of hypnoid phenomena" (91). "He is not troubled about her general health. . . . . His attention is concentrated upon certain special physiological functions, of the normal exercise of which he takes care to be assured. . . . . He has for several years formally laid his ban upon every kind of mediumistic exercises at certain very regular periods" (133 f.). Leopold is thus the personification (and M. Flournoy offers a reasonable account of the building-up of his personality: 91 ff.) of "scruples more or less vaguely felt, certain hesitations or apprehensions, inhibiting feelings or tendencies,"—of "instinctive feelings and emotional tendencies,"

—characteristic, to some extent, of the feminine mind in general, but culminating in the present instance in "knowledge and prevision of the most intimate phenomena of the organism" and an unusually refined coenæsthetic sensibility. In the light of this analysis it is intelligible that, in her ordinary social relations, 'Mdlle. Smith'—apparently, whether she is under the immediate direction of Leopold or not—shows a modesty and reserve that amount almost to prudery (89 f., 28, 131). The sexual instinct finds its satisfaction—in an idealized and platonic way, but none the less really—in the occurrences of the trance-life (8; 115; 287 f., 289 f., 293, 341).

I have no intention of following M. Flournoy through his analysis of the three 'cycles,' the Martian, the Hindoo, and the Royal. He has done his work with extreme patience and acumen. Indeed, when one remembers the almost infinite possibilities of suggestion to which 'Mdlle. Smith' has been exposed on the confines of her trances (a point sufficiently emphasized by the author: 49 ff., 93, 146, etc.), one marvels at the high measure of success in explanation that M. Flournoy has attained. Few psychologists will dispute his conclusion that the 'secondary personalities' "do not exist outside of Mdlle. Smith," but have their genesis within her mental life. And even those who, like the present writer, have no great affection for the 'subliminal self,' but rather believe with Galton that the terms 'individuality' and 'personality' are apt to be psychologically misleading,—even they will readily admit that M. Flournoy's methodical study proves the high value of subconscious imagination as a working hypothesis. As for the outcome of the author's study of the 'supernormal,'—"I believe I have actually found a little telekinesis and telepathy [though the appearances of lucidity and spiritistic communication are explicable in terms of the hypnoid imagination],"—the critic can only note that his attitude to the phenomena is rather that of "I can't disprove the things: let them rest for further investigation" than that of positive acceptance (381; 389; 396, 401; 407, 13; 424; 445).

In his conclusion, M. Flournoy lays just emphasis upon the difficulty of the "neuropathological problem of mediumship." "To endeavor to fix the connections of mediumship with other functional affections of the nervous system, it would first be necessary to possess exact intelligence on a number of important points still enveloped in obscurity." We must understand "the phenomena of periodicity, of meteorological and seasonal influences, of impulses, and of fatigue;" "the relations of equivalence and substitution between the various modalities of automatism;" "the effect of spiritistic seizures, and especially of that of the seances, upon nutrition and denutrition (variations of temperature, of urotoxicity, etc.)." These are wise words; and one can only express the hope, with the author, that "a near future will establish some good mediums and their observers in practical conditions favorable to the elucidation of these various problems, and that the day will come when the true place of mediumship in the framework of nosology will be discovered" (441 f.).

The translation, save for a few slips due more to hasty work than to misunderstanding, seems to be accurately made. Unfortunately, Mr. Vermilye cherishes the conviction—surely erroneous—that any phrase in the English language, however constructed, can be grammatically added to any other phrase by means of the magic connectives "and which." The application of this principle—which, on M. Flournoy's model, may be termed the principle of Mrs. Sarah Gamp—leads to sentences like the following (italics mine). "The spontaneous phenomena of this category . . . . can be subdivided into two classes, according to their direct attachment to the personality of Leopold, or

their not belonging to any distinct personality, *and which* only express in a vivid manner the result of the normal working . . . . of the faculties of memory and of reason" (58). But, if the translation has faults of style, the translator's Preface adopts a tone which is simply inexcusable. To suggest that "the scientific investigation of psychic phenomena may succeed in proving *the preamble of all religions*" (ix) is to put the prospective reader in a frame of mind the very opposite of that which the reading of the book demands, and which the writer of the book desires. Future editions of the work should be supplied with an index. E. B. T.

*L'année psychologique.* Publiée par A. BINET, avec la collaboration de H. BRAUNIS, TH. RIBOT, etc. Sécrétaire de la redaction, V. HENRI. Quatrième Année, 1898, pp. 849; Cinquième Année, 1899, pp. 902. Paris: Librairie C. Reinwald; Schleicher Frères, Editeurs.

These two volumes amply sustain the reputation that their predecessors have won for M. Binet and his coadjutors. Vol. IV opens with an article by MM. Binet and Vaschide upon psychology at the primary school. This is followed by seventeen papers, from the pens of the same authors, dealing with the measurement of muscular strength, respiration, etc., of school children. A critique of the oval hand-dynamometer and of the Mosso ergograph will be extremely useful to the experimental psychologist. M. Binet then contributes three essays upon topics akin to those mentioned, and M. Vaschide writes upon the influence of prolonged intellectual work upon the rapidity of the pulse. M. Bourdon discusses the application of the graphic method to the study of the intensity of the voice, and the results of recent investigations of the visual perception of depth. M. Leclère's "Description of an Object" is an interesting piece of individual psychology.

In Vol. V, M. Joteyko gives a 'revue générale' of muscular fatigue, and M. Henri of the 'muscular' sense. M. Binet writes on suggestibility, as a factor in individual psychology; and this author, with MM. Henri and Larguier, deals again with the questions of intellectual work and its measurement. There are, further, articles by M. Bourdon, on horizon illusions; by M. Clarapède, on stereognostic perception; by M. Clavière, on colored hearing; by Professor Zwaardemaker, on olfactory sensation; by M. Marage, on the phonographic study of vowel sounds and on cephalometry; by M. Blum, on paidology; by M. Larguier, on dynamometry; by M. Demeny, on chronophotographic apparatus; and by M. Manouvrier, on anthropological cephalometry. A good and varied programme!

Both volumes contain, besides these original articles, abstracts and reviews of the most important publications of their respecive years, and a complete bibliography.

*L'Année Psychologique*, publiée par ALFRED BINET. Sixième Année. Schleicher Frères, Paris, 1900. pp. 774.

This most welcome volume differs from its predecessors in giving the first 492 pages to original articles, three of which are by Binet, and all of which are interesting and valuable.

The bibliography itself occupies 130 pages, so that only 123 are devoted to actual digests of a limited number of articles. These are so good that, despite the excellence of the original contributions, many will regret that more space was not given to this department of the Année.

*L'Évolution du droit et la conscience sociale.* Par L. TANON, président à la Cour de Cassation. Paris, F. Alcan, 1900. pp. 166. Price, fr. 2.50.

All legal rules and institutions have been determined, on the one hand, by utility and the conditions of living, and on the other by the successive stages by the developing social consciousness. The term "common good" is that which best covers the facts; it enables us to oppose to the principles of "natural law" a positive conception, of legal value, and according with the data of history.

*David Hume, moraliste et sociologue.* Par G. LECHARTIER. Paris, F. Alcan, 1900. pp. 278. Price, fr. 5.00.

Hume's work has at least the negative value of combating the exaggerated pretensions of the speculative reason. But it also forms a positive propædeutic to a true philosophy. M. Lechartier opens his book with a brief biography of his subject. Then follows an account of the ethical problem before Hume, whose own contribution to philosophy is considered in two chapters, under the titles of *Philosophie théorique* and *Philosphie pratique*. The volume concludes with a translation of "A Dialogue."

*Leitfaden der physiologischen Psychologie, in 15 Vorlesungen.* Von TH. ZIEHEN. Fünfte, theilweise umgearbeitete Aufl. Jena, G. Fischer, 1900. pp. v, 267.

The text of this fifth edition differs only in minor points from that of 1898. The references have been brought up to date, and four new cuts appear (Fig. 7, section through a *Papilla foliata* of a rabbit; Fig. 12, schema of the structure of the organ of Corti; Fig. 13, schema of the structure of the retina; and Fig. 23, schematic representation of the affective tone of an idea). Fuller treatment is accorded to the process of Recognition in Lect. X. By the use of a somewhat smaller type, the book has been kept down to its former size.

*Mind and Body.* By A. BAIN. pp. 49. Price, c. 15.

*Illusions of the Senses.* By R. A. PROCTOR. pp. 41. Price, c. 15.

*Hypnotism: its History and Present Development.* By F. BJOERNSTROEM. pp. 126. Price, c. 30.

*Body and Mind, with Other Essays.* By W. K. CLIFFORD. pp. 47. Price, c. 15.

*Mental Suggestion.* By. J. OCHOROWICZ. pp. 96, 369. Price, $1.20.

Nos. 13, 82, 113, 145, 152-8 of the Humboldt Library of Science. New York; The Humboldt Publ. Co., 1880-92.

We are glad to call the attention of our readers to the psychological volumes of the Humboldt Library. The parts are published in pamphlet form, with a page of about 23 by 15 cm. The earlier numbers were printed in double columns and (apparently) from photographic films: the text is small and broken. The later volumes have a single-line page, and the text is clear and good. In supplying a book like Clifford's "Essays" in this form and at this price, the publishers have deserved well of the psychological community.

*Sensation et Mouvement, Études expérimentales de psycho-méchanique.* Par CH. FÉRÉ. Deuxième édition, revue. Paris, F. Alcan, 1900. pp. 170. Price, fr. 2.50.

The first edition of this well-known little book was published in 1887. The text has now been revised, and minor changes in expression made at various points, while new notes and references have been added.

*Questions de Morale, Leçons professées au Collège libre des Sciences sociales.* Par G. BELOT, M. BERNÈS, F. BUISSON, A. CROISET, V. DELBOS, A. DARLU, E. FOURNIÈRE, P. MALAPERT, G. MOCH, D. PARODI, G. SOREL. Paris, F. Alcan, 1900. pp. vii, 331.

The ethical addresses delivered at the Collège libre des Sciences sociales in 1899 were collected in a single volume, under the title *Morale sociale*. Encouraged by the success of this book, the Council of Direction now issue the addresses of 1900, as Vol. VIII of the Alcan Library of Social Science. Twelve lectures are printed in full. M. Sorel writes on science and morality; M. Moch on the subject of the book prepared by him in collaboration with M. von Egidy,—'L'Ère sans violence;' M. Darlu on Christian ethics and the consciousness of to-day; M. Sorel, again, on the moral factors in evolution; M. Delbos on Kant and the science of ethics; M. Croiset on Greek ethics; M. Bernès on the conditions of action; M. Parodi on reason and instinct in morality; M. Belot on luxury; M. Fournière on Guyau's ethics; M. Malapert on social justice; and M. Buisson on moral and religious education. The volume contains, further, abstracts of the addresses delivered by MM. Boutroux and Croiset on the opening of the School of Ethics, Nov. 27, 1899.

*Über Psychologie der individuellen Differenzen (Ideen zu einer Differentiellen Psychologie)*, von L. WILLIAM STERN. Schriften der Gesellschaft für Psychologische Forschung. Leipzig, Verlag von Johann Ambrosius Barth, 1900. pp. 144. Price, M. 4.50.

The book is a valuable and needed résumé and critique of the work already done in a subject which has grown up in its present form as a protest against the indiscriminate massing together of the results of psychological experimentation, and an examination of its aims and methods. It is divided into three parts, the first treating of the nature, tasks and methods of differential psychology; the second of some of the fields of mental differentiation and their experimental investigation; and the third devoted to a select bibliography of the subject. The author's exclusion from the bibliography of a mass of more or less available material was doubtless due to the necessity of curtailment on account of lack of space. Practically a complete bibliography of differential psychology would be a bibliography of experimental psychology.

Chap. I of the work discusses differential as opposed to general psychology, psychological types, individuality, the normal and abnormal. As regards terminology, the name "differential psychology" is preferred to that of "characterology" (Bahnsen), "Ethology" (Mill) or "individual Psychology" (Binet, Henri, Kräpelin, and others). "Characterology" and "Ethology" are considered too narrow. "Individual psychology" is already employed with another meaning as opposed to folk and social psychology. Chap. II reviews the methods of differential psychology: introspection; observation; the psychological studies of history and poetry (where observation is directed upon secondary sources), and of the evolution of culture, the questionnaire or census method (*Massenprüfung*, *Enquête*), and experimentation. He thinks the questionnaire method should be used with the greatest possible care, and that the *Prüfling* (he prefers this term to reagent or *Versuchsperson*) should be, if possible, a trained psychologist. The author seems, however, quite to overlook the suggestive value of the questionnaire in opening up new fields of investigation. Under the method of experimentation, the author discusses what a "mental test" is and declares that an hour and a half, as given by Binet and Henri to the testing of the memory, imagination, attention, etc., of a

single individual is entirely inadequate, a point that may be regarded as well taken or the reverse according to the purpose for which the examination is made. Chap. III treats of natural ability to distinguish and of active sensibility. Here the question as to whether individual differences rest upon sensation or the higher mental activities is taken up. When one remembers the significance of this question as regards the extraordinary powers exhibited by certain individuals in a given direction, one regrets with the author that Binet and Henri did not demonstrate their formula "the higher the psychical function, the greater the individual deviations," by experiment, although there is perhaps sufficient empirical material already at hand to justify the tentative use of the formula. Chap. IV takes up the determination of the general perception type and the formal perception type, meaning by the latter the type as regards the perception of time and space relations. Chaps. V–VII are devoted to the excellence of the memory and the differentiation in memory content; to association; and to apperception types. Chap. VIII deals with continuous and momentary distraction of the attention, with distractability and energy of attention, and with distractability and soundness of sleep. The topics of Chaps. IX–XII are: ability to make combinations; definiteness and reliability of the judgment, suggestibility of the judgment, subjective and objective types of judgment; reaction types; and the feelings. In Chap. XIII (on Psychic Tempo) the author reports the results of some preliminary experiments which go to show that each has a time in which he naturally acts and reacts. In Chap. XIV day and night curves of psychical energy and the characteristic quality of work are considered. Like so many German books, this book would be much increased in value by an adequate index. LILLIE J. MARTIN.

*Prison Laboratories.* By C. R. HENDERSON. American Journal of Sociology, Nov., 1900. pp. 316-323.

The author embodies the report of a committee appointed in 1899 by the National Prison Association "to consider the wisdom of establishing laboratories in a limited number of prisons." The committee reported favorably, recommending laboratories which shall be permanent, under the control of well-trained experts, with the practical motive of studying the personal, hereditary and environmental influence of the prisoners. Records are to be kept of body measurements, mental activity in attention and perception, in making associations, comparisons, and in reasoning, a work which has not been done heretofore. It is hoped that if the causes of crime are not found, at least a means of its amelioration may be. EZRA ALLEN.

*Individual Tests of School Children.* By E. A. KIRKPATRICK. Psychological Review, VII, 1900, 274-280.

In studying tests of general mental ability Professor Kirkpatrick has discovered that the study of 500 children of grades 1-8 for keenness of sensory-motor reactions showed that the curve rises to the sixth grade or so, and then remains stationary, or declines. He infers that there must be different tests for different stages of development, and that quick reaction in these matters may be no criterion of general ability in college students. M. F. LIBBY.

*The Old and the New Magic.* DR. PAUL CARUS. Open Court, Vol. XIV, 1900. 333-348, 422-437.

The old magic sought to transcend human knowledge by supernatural methods, by the assistance of invisible presences. This idea, strong in the Middle Ages, still exists. In primitive society religion is magic. The manhood of man is to be gained by science through

the conquest of all magic. Magic as superstition is doomed. As art it will live. A knowledge of conjuring tricks is desirable, and the author advises its communication in the education of children.

In the observation and detection of magical tricks scientists are likely to err widely. Prof. Zöllner failed to notice the trifling acts of the medium, which were really the important things. The psychological problem in the case of the extravagant tales of the wonders of foreign magicians is as to the state of mind of the narrator. The triviality of a number of these tricks are shown. The article is a good popular presentation. NORMAN TRIPLETT.

*The Modern Occult*, by PROF. JOSEPH JASTROW. Pop. Sci. Monthly, Sept., 1900.

The author shows the relation of the modern occult to ancient occultism, and discusses the present-day phases of Theosophy, Spiritualism and Christian Science, with a passing mention of Alchemy, Astrology, Phrenology, Palmistry and Divine Healing. Christian Science, so-called, is perhaps calculated to exercise a wider influence than the other cults.

The reason for the development of these occult beliefs are: ignorance, a somewhat feeble cast of mind that does not perceive the errors of false logic, and tends naturally to superstition, and an undue anxiety concerning one's own personality. The antidote lies in the diffusion of exact knowledge. MARGARET K. SMITH.

*La psychologie de 1899 à 1900*, par TH. RIBOT. Revue scientifique, T. XIV, 353-356. Sept. 22, 1900.

In this presidential address before the recent Psychological Congress *Ribot* reviews Psychology from 1889-1900. In 1889 the interest was chiefly in hypnotism, mind-reading, etc.; in 1892 in psychophysics, nervous system, and exact experiment; in '96 in *everything* that could help in any way; psychophysiology, psychology of normal and abnormal persons, and comparative psychology. In 1900 we begin to find that the psychologist must *interpret* what the neurologist and physiologist discover. In memory and association good work is being done in study of children and unconscious association, the latter amid keen controversy. The study of attention and motor and sensorial reactions tends to show that reactions depend on individual constitution. Whether attention increases intensity and clearness of representations is still doubtful. He hopes for more theses on the emotions, and on complex processes generally, such as reasoning and imagination. Accuracy can be got, by a firm empirical basis in art, anthropology, linguistics, etc. Sound work is being done in genetic pyschology on this basis, and much may be hoped in psychology of character, and of the tribe. Germans and Americans lean to psychophysics, French and Italians to abnormal psychology, and English to introspection. We do not at present want general outlines of psychology so much as monographs. M. F. LIBBY.

*La question des méthodes en psychologie*, par M. GUIDO VILLA. Revue scientifique, T. XIV, 357-362, 22 Sept., 1900.

In this Villa discusses methods in psychology. He also advocated freer scope in handling the vast subject matter of human and animal history from the point of view of psychology, and praised the American methods as illustrated by the non-mathematical yet empirically-grounded and scientific interpretations of James and Baldwin, rather at the expense of the timorously accurate German methods, while admitting that the latter had overthrown the introspective literary dilettantism of the 18th Century. His whole argument is to the effect that

having learned the meaning of scientific methods of introspection it is time to move on toward interpreting the history of the human mind wherever we get facts about it, instead of confining ourselves to such facts about it as can be handled with mathematical precision.

M. F. LIBBY.

*La Philosophie de H. Taine*, par GIACOMO BARZELLOTTI. Tr. from the Italian by Auguste Dietrich. F. Alcan, Paris, 1900. pp. 448.

This is an exposition of Paine's philosophy and his method, and seeks to show that his genius was an eminent expression of the Zeitgeist, both intellectual and æsthetic, that as such his systematizations made not only for mental economy, but that his thoughts were the seats of other harvests.

*Les Dilemmes de la Métaphysique Pure*, par CHARLES RENOUVIER. F. Alcan, Paris, 1901. pp. 288.

The dilemmas are—the unconditioned and the conditioned, substance, the all or function of phenomena, the infinite and the finite, determinism and liberty, the thing or the person. These are all reduced to one alternative, to one thesis and antithesis, viz., being or non-being.

*Grundzüge der Psychologie*, von HUGO MÜNSTERBERG. Vol. I. J. A. Barth, Leipzig, 1900. pp. 565.

This volume is dedicated to the author's "dear colleague in Harvard University. William James, in sincere reverence and heartiest friendship." This general part, which deals with the principles of psychology, is to be followed by a special part treating the facts. The first part treats the principles of individual, and the second those of sociological psychology. We are promised that the whole will be no objective text-book restating the generally well known facts of the science for the writer's purpose is more subjective. The author will discuss rather than present; will sift out and connect rather than inform; so that from the rich manifoldness of the data, real and unitary features shall appear. His effort is, therefore, at the bottom philosophical. This is especially true of this introductory volume which is more or less complete in itself, treating of fundamental ideas, presuppositions, limitations and ideals of psychology. It is a book of war (Kampfbuch), which in an unphilosophical age takes up the gauntlet of idealism against naturalism. The positivistic view of the world, which has one of its strongholds in psychology, must be met, for it is striking its insidious roots into our social consciousness, education, art, ethics, and morals. It cannot be overcome by despising or denying empirical research. Three, and those perhaps the most important chapters, are essentially the same as in the author's Psychology and Life. His general position is a kind of synthesis of Schopenhauer, Fichte, and scientific psychology. As far as we have read the second and third parts, they are more or less implied in the author's English work. We hope, however, to recur to these more at length later, especially as the standpoint, which the author ably represents, is so radically different from the idealism to which the writer of this note no less fervently holds.

*Text-book of Physiology*. Edited by E. A. Schäfer. Vol. I, pp. 1036. Vol. II, pp. 1365. Young J. Pentland, Edinburgh; The Macmillan Co., New York, 1898 and 1900. Price, $8.00.

Professor Schäfer and his twenty co-laborers have here produced a monument to English physiology which, although less extensive than Hermann's Handbuch of twenty years ago, is no less well devised and representative. In comparing the two, one is struck by a progress

hardly less marked than that which separated Hermann from the old four volume Handwörterbuch of Wagner in 1846. While this work owes as all must very much to Hermann, it is a striking reminder of the great progress which physiology has made in the last two decades. Foster's first edition marked an epoch in physiology in English speaking lands, and was a marvel of individual industry and ability, but the field has distinctly outgrown the ken of any one representative, and henceforth every adequate presentation of what is embraced under the term physiology must be co-operative.

By this method the authors are enabled to present in precise form the contents of many hundreds of original papers which are referred to in the notes, so that for general purposes the necessity of referring to first sources is superseded, while for those who need to do so this work is facilitated. The enormous literary work involved in such a book is one of its striking features. The volumes are well supplied with indexes, the first volume having seventy-two and the second one hundred and four pages, while there are five hundred and forty-one cuts and many tables.

Psychologists will be especially pleased with the chapters on nerve, by Gotch; on the nerve cell and the cortex, by Schäfer; on cutaneous sensations and muscle sense, by Sherrington; on vision, by Rivers; the ear and vocal sounds, by M'Kendrick and Gray; and on the senses of taste and smell, by Haycraft. In most cases these later writers have well availed themselves of the experimental work of psychologists who, if they often find that articles they would like to see considered, have not been noticed as sometimes occurs, will not be too critical with a gift horse. Every working psychologist will need this book at hand until it is superseded by a better.

It is easy to criticise, but we cannot forbear expressing our regret that the subjects of generation and reproduction, as well as the physiology of the cell generally considered in such treatises, have been omitted, partly because the subjects involve morphological methods, and partly because they are so important that they would swell the size of the volumes unduly.

*Diffusion of the Motor Impulse.* WISSLER AND RICHARDSON. Psy. Rev., Jan., 1900. pp. 29-38.

In this paper experiments on arm muscles are described, having for their object the determination of the order of secondary contractions when the *abductor indices* and the biceps are respectively exercised by a series of movements. By means of tambours and kymograph, records of muscular contractions of two young men were obtained which show that when the motor discharge is directed to an extreme accessory muscle of the arm, diffusion is primarily to the muscle directly innervated and secondarily to adjacent related muscles in the order of their distance anatomically from the muscle innervated. The experiments also show diffusion to be downward as well as upward, and lead to interesting hypotheses respecting "cross education."

F. H. SAUNDERS.

*Effets du travail de certains groupes musculaires sur d'autres groupes qui ne font aucun travail.* KRONECKER AND CUTTER. Comptes Rendus, T. CXXXI, No 10 (3 Sept., 1900). p. 492.

This communication reports the results of experiments to determine the effects on the power of the biceps group of muscles of exercising the lower limbs in mountain climbing. The tests made after climbs requiring varying time and exertion clearly indicated in all cases gains in power in biceps muscles. The writers express the opinion that the effect is due to increase of circulation of blood or lymph.

F. H. SAUNDERS.

*Heredity and Human Progress*, by W. DUNCAN MCKIM. G. P. Putnam's Sons, New York, 1900. pp. 283.

The dark side of human existence; the cause of human wretchedness; the defective classes; a remedy; and a consideration of objections against it, are the chief features of this book. The most striking and central idea is that artificial selection should help the elevation of the human race, partly by restricting reproduction by those organically very weak or vicious, and doing this, to use his language, "by the surest, the simplest, the kindest, and most humane means of preventing reproduction among those whom we deem unworthy of this high privilege" by "a gentle and painless death." "This should be administered, not as a punishment, but as an expression of enlightened pity for the victims too defective by nature to find true happiness in life, and as a duty toward the community and toward our own offspring." "The essential feature of the plan is the gentle removal from this life of such idiotic, imbecile, and otherwise grossly defective persons as are now dependent for maintenance upon the State, and of such criminals as commit the most heinous crimes, or show by the frequent repetition of crimes less grave, by their bodily and mental characters, and by their ancestry, that they are hopelessly incorrigible." "The majority of epileptics would require extinction; but those in whom the disease has apparently been caused by injury or by some removable condition, and whose families give indication of but little degenerative taint." "Respecting habitual drunkards, the degree of addiction to drink which should necessitate extinction would best be decided through a physical and mental examination," not in the quantity of liquor consumed in a day, nor in the duration of the habit. Criminals should be killed not so much for the injury dealt to society by any single act as their own dangerous quality. Murder is still the greatest crime, but here there should be discrimination. Nocturnal house breakers should be extinguished and all criminals who are idiots, imbeciles, epileptics, habitual drunkards, and insane, and all whose record in reformatory or prison shows them to be hopelessly irreclaimable. In every case the individual should undergo thorough examination, and his life should be taken only after due process of law. The method is not difficult. "In carbonic acid gas we have an agent which would instantaneously fulfill the need." To be sure, he says, the number of individuals is very great. In the United States there are now on the average thirty-eight murders a day, so that we live in the shambles, and this proportion is steadily increasing. About 14,000 of our fellow citizens die violent deaths annually. This might require a change of penal legislation, and even of some State constitutions, but "those who are shocked by the remedy here suggested, I would say that I too am shocked by it, but to be it seems a hard necessity laid upon us because our fathers failed to perceive their duty in this regard and to assume their proper burden."

*The Life, Unpublished Letters, and Philosophical Regimen of Anthony, Earl of Shaftesbury*. Edited by Benjamin Rand. Swan Sonnenschein and Co., London, 1900. pp. 535.

We have here a sketch of the life and the unpublished letters of the Earl of Shaftesbury, the English moralist, based on the Shaftesbury papers deposited in the Record Office in London. Fowler in his well known recent work on Shaftesbury and Hutcheson expressed the belief that these papers would repay more careful investigation than he was able to give them. The work entitled "Philosophical Regimen" (267 pages) discusses natural affection, providence, shame, reputation, friends, self, body, passions, pleasure and pain, character, maxims,

philosophy, and many other topics in a way so interesting that the editor declares that they contain "one of the most remarkable unpublished contributions of modern times in the domain of philosophical thought." His life is written by his son, the fourth Earl of Shaftesbury, and the second division contains his unpublished letters beginning at the age of eighteen up to his death, in 1713. The letters written to or about young men have a unique charm, while historians will welcome his political letters. His philosophy, as presented in "Characteristics," was largely stoical, but we have here a new and brilliant presentation of that system which shows how intoxicated he was with the idea of virtue. Indeed, since Marcus Aurelius, there perhaps has not been so strong an expression of stoicism as is contained in this "Regimen," now here published for the first time.

*Fruitfulness*, by ÉMILE ZOLA. Translated and edited by Ernest A. Vizetelly. Doubleday, Page and Co., New York, 1900. pp. 487.

This is a translation of one of Zola's most important stories, Fécondité, which may be of some interest to psychology. The writer was impressed with the diminishing natality in France, and here undertakes to characterize two families with their environment—one which was growing rapidly and with many children in its various branches; and the other, although more favorably situated, which was slowly dying out. It is all wrought up with his usual dramatic interest.

*La Philosophie de Nietzsche*, par HENRY LICHTENBERGER. F. Alcan, Paris, 1900. pp. 195.

The author has met a long felt want in giving us a comprehensive picture of Nietzsche's character, his intellectual emancipation up to 1878, his philosophy, most of which was written during the next ten years, and the positive and negative traits of his system. It also contains a convenient appendix and a bibliography—first, of all Nietzsche's writings, and secondly of the more important treatises on them.

*Researches on Mimicry on the Basis of a Natural Classification of the Papilionidæ*, by ERICH HASSE. Translated by C. M. Child. Erwin Nägele, Stuttgart, 1896. pp. 154.

The author goes through the butterflies and undertakes to describe each known case of mimicry in the various species and varieties. Although the work is technical and addressed chiefly to experts in natural history, its subject makes it of great interest to the general reader.

*Liebe und Ehe und ihr Naturrecht*, von RICHARD FUGMANN. Wilhelm Besse, Leipzig. pp. 128.

This book treats celibacy, chastity, monogamy, health, disease, who ought not to marry, hints for healthful marriage; and its motto is "chastity is the beginning of wisdom." It belongs to a large class of publications which may and may not do good, which are neither scientific or philosophical nor obscene, but which are sentimental, mystic and weak, and just enough risqué in title and content to tempt customers of a German railroad station news-stand. This journal has had, from time to time, about a dozen and a half books of this class that belong to the limbo where Kipling's Tomlinson found himself doomed, whom neither the good nor the bad place would receive. All are sure to quote or characterize briefly from Schopenhauer, Plato, Rousseau, Goethe, and perhaps Byron and Nietzsche. A small ingredient of their content is from Darwin, and they speak mysteriously of the morbid sex literature, but avoid its abysses. They poetize about true love and marriage, and the nameless horrors of certain diseases.

*Die Transszendentale und die psychologische Methode*, von MAX F. SCHELER. Dürr, Leipzig, 1900. pp. 181.

This interesting and timely work is a criticism of the transcendental method of considering space, time, personality, and cause, and the latter part is a statement by contrast of what he calls the psychological method. The former, he urges, can never solve the problems of philosophy. The noological method, which the author proposes, is based upon the fundamental ideas of work and consciousness. He declares that the latter cannot be considered as a product of development.

*Interpretation of Poetry and Religion*, by GEORGE SANTAYANA. Charles Scribner's Sons, New York, 1900. pp. 290.

The understanding, imagination and mysticism, Homeric hymns, the dissolution of paganism, the poetry of Christian dogma, platonic love and some Italian poets, the absence of religion in Shakespeare, the poetry of barbarism, Emerson, a religion of disillusions, the elements and functions of poetry are here treated. To this super-æsthetic and hyper-subtle mind, which is itself an exquisite illustration of the emasculating effects of an habitual atmosphere of epistemology. Browning and Whitman illustrate the poetry of barbarism; love is far more platonic than physical; belief at its best is a kind of poetry; and everything is falsetto and unreal. It would be difficult to find a better illustration of the devirilized effects of transcendental and idealistic habits of thought than in these pages, which are so chastened and refined that all vitality seems to have gone out of them. His is a world without color, where feeling, impulse and emotion merely cadence the forms of thought, and where the grasp on reality is so feeble that the whole has a charm that is supernal and afar. So bloodless a writer, whose mentality is so far removed from all storms of passion, and whose very beliefs lack fervor, from which all trace of spontaneity has long since vanished, seems to us a good illustration of decadence, but if so, degeneration never had a greater charm.

*Education and the Philosophical Ideal*, by HORATIO W. DRESSER. G. P. Putnam's Sons, New York, 1900. pp. 255.

This, we understand, is the tenth book of this prolific young mystic. We notice in this no telepathy, theosophy, or mind cure in their familiar forms, but there is a subdominant tone throughout that suggests that the author has long frequented the adyta of adepts in these lines. He believes in his new point of view in educational ideas in equanimity, the subconscious mind, the spiritual ideal, the expression of the spirit, its ministry and that of pain and evil, in organic perfection, philosophical ideals and in immortality, and tells us also with earnestness and with quotations from Emerson, Amiel, Lowell, Froebel, John Fiske, Mr. Salter, Browning, and even Henry Wood, from which points the experienced reader can calculate his orbit although he may not have time to drift with him through all the rather pleasant meanderings of his pages, but a strenuous reader will wish they were better, or if that could not be, that they were worse.

*Studies from the Yale Psychological Laboratory*. Edited by Edward W. Scripture, 1899. Vol. VII, pp. 108. Yale University, New Haven, Conn., 1900.

This volume contains a major article entitled "Researches in experimental phonetics," and a minor one on "Observations on rhythmic action," both by Dr. Scripture.

*Proceedings of the Society for Psychical Research.* Oct., 1900. Vol. XV, Part 38.

This number is devoted to the divining rod, with many illustrations and candid presentation of evidence both ways.

*Essai sur l'Imagination Créatrice*, par TH. RIBOT. F. Alcan, Paris, 1900. pp. 304.

This study is worked out with the author's usual intelligence, lucidity, and sense. The intellectual, emotional, and subconscious faculties of imagination are distinguished from each other as well as from its organic conditions. The development of the imagination in animals, children, primitive man and myths, up to the higher forms of invention, with certain general laws of its development are next described. The following are its principal types—plastic, diffluent, mystic, scientific, practical, mechanical, commercial, and Utopian.

*Variétés Philosophiques*, par J. P. DURAND (DE GROS). F. Alcan, Paris, 1900. pp. 333.

The traces of metaphysics in natural medical sciences interest this author first. He then discusses the two species of cells in the nervous centers, Taine's ontology, Fourier's psychology, polyzoism, free will, the unconscious, pantheism, creation, finality, and miracles.

*L'Imagination et les Mathématiques selon Descartes*, par PIERRE BOUTROUX. F. Alcan, Paris, 1900. pp. 45.

We have here a collection of quotations and paraphrases of mathematical authors since Descartes to enforce the precept that the imagination is an essential part of the outfit of a mathematician, both for knowing and for demonstrating in his field.

*The Conception of Immortality*, by JOSIAH ROYCE. The Ingersoll Lecture, 1899. Houghton, Mifflin and Co., Boston, 1900. pp. 91.

The author sums up his case by conceiving the world as a rational whole, a life in which the divine will is uniquely expressed. Every aspect of this life must, therefore, be unique and mean something that can only get an individual expression. But while we want to love and know there are in our present form of consciousness no true individuals to be found, yet because we are one with God, our lives must in the end attain individual significance, so that we are only hints of individuality not yet revealed. Hence the real individuality, which we mean to express, must have its final conscious utterance in a life that is conscious, and though independent of space and time, is continuous with our present fragmentary, flickering being.

*Nietzsche's Ästhetik*, von JULIUS ZEITLER. H. Seeman Nachfolger, Lepzig, 1900. pp. 308.

At last we have a good digest of Nietzsche's views in clear language, which ought to be translated, treated under the metaphysics of æsthetics, art psychology or criticism, and physiological æsthetics. The author is intent mainly upon digest and characterization, and both quotation and criticism take a secondary position.

*The Individual. A Study of Life and Death*, by NATHANIEL S. SHALER. D. Appleton and Co., New York, 1901. pp. 351.

The chapters are—the individual's place in the universe, organic individuals, duration and nature of individuality, place of organic life, growth of sympathy, expression of individuality, appreciation of others, fear and valor, attitude of man and society to death, relation of parent to child, old age and its utilization.

*Man and His Ancestor. A Study in Evolution*, by CHARLES MORRIS. The Macmillan Co., New York, 1900. pp. 238.

Since Darwin, we are told the subject of man's evolutionary origin cannot be said to have been dealt with for itself alone, but Mr. Morris treats man's ancestry, relics and ancient man, the progress from quadruped to biped, freedom of the arms, development of intelligence and language, how the chasm was bridged, first stage in human evolution, the conflict with nature, warfare and civilization, the evolution of morality, man's relation to the spiritual.

*La Respirazione nelle Gallerie e l'Azione dell'Ossido di Carbonio*, da ANGELO MOSSO. Tip. Treves, Milano, 1900. pp. 322.

Professor Mosso has brought all the marvellous resources of his genius for apparatus to bear here for studying the action of the lungs in the great railway tunnels in Italy, and describes the pressure, state of the blood. action of the heart, curve of fatigue, etc. The work was undertaken at the suggestion of the Minister of Public Works.

*Apes and Monkeys, their Life and Language*, by R. L. GARNER. With an Introduction by Edward Everett Hale. Ginn and Co., Boston, 1900. pp. 297.

Last spring Mr. Garner gave the members of the Psychological Seminary at Clark University great pleasure in detailing his acquaintance with monkeys. A sagacious publisher upon hearing him forthwith engaged him to amplify it to a book, which he has here done. Whatever we may think about the soundness of the author's theories or the range of his knowledge, he has certainly here made an interesting and valuable book, which is also gotten up in very presentable style.

*An Introduction to the New Testament*, by BENJAMIN W. BACON. The Macmillan Co., New York, 1900. pp. 285.

After the usual discussion of the history of New Testament thinking since Strauss and Renan, the author traces the growth of tradition and formation of the canon, and then turns in detail to the Pauline epistles. The third part is devoted to the Catholic epistles, viz., Peter, James, Jude, and 2 Peter; the fourth part to the historical books, synoptic tradition and writers; and the last to John's gospel and the apocalypse.

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